

Orthography in Ancient Hebrew Inscriptions

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The recovery of the phonetic shapes of Hebrew words attested in inscriptions from the biblical period is often confounded by uncritical retrojection of Masoretic orthoepy onto ancient spellings. Sound empirical method works with the expectation that ancient Hebrew spelling was phonetic, at least in intention. On the basis, the development of various word classes can be traced with more precision.

The recent publication of a grammar of inscriptional Hebrew¹ promised to meet a long-felt need. The results, however, are disappointing. In particular, the discussion of spelling — under the rubric of “phonology”! — represents a step backward rather than progress. The section “*Matres Lectionis* in Epigraphic Hebrew” (#2.3 [pp. 49-74]) is very confused.

Ancient Hebrew Vowel Letters

Without going into all the details, we observe that the use of the term *matres lectionis* is anachronistic, and gets medieval Masoretic spelling policies mixed up with the ancient use of consonants — three only, *waw*, *yod*, and *hēʾ*, not *alef* — as vowel letters. We are not aware of a single specimen of the ancient use of *alef* distinctively as a vowel letter in the strict sense, that is, where it is not etymological. To assume any etymological *alef* was silent after a vowel in a particular instance begs the question by projecting Masoretic pronunciation backwards one thousand years or more. Even though Dr. Gogel calls *alef* a *mater lectionis*, already in use in epigraphic Hebrew, she equivocates, conceding that “all of these are apparently histor-

¹ Gogel 1998.

ical writings”.² In early Aramaic the attestation of 𐤀𐤌 in the Tell Fakhariyeh bilingual statue inscription was a surprise; but there is no way of proving that the *alef* in such words was not pronounced as a glottal stop.³ Zevit’s statement that *alef* is “not etymological” in 𐤀𐤌⁴ is probably true in the strict historical sense.⁵ But that does not mean that the *alef* in that word was introduced as a vowel letter and never represented a consonant sound. The almost universal spelling of this negative particle in Hebrew and Aramaic is 𐤀𐤌. All known instances in ancient Hebrew inscriptions have *alef*.⁶ The form of this particle in cognate languages, such as Ugaritic, without *alef*, does not necessarily prove that the consonant was not etymological. But, even if it points that way, it does not follow that a secondary *alef* can only be a vowel letter. Whatever the history of the vowel in **la* — whether it was lengthened to *ā* in time to experience the Old South Canaanite sound shift to *ō* or whether the short vowel was preserved by using a glottal stop to close the syllable, so that the change to *ō* occurred at a later stage after the lengthening followed (compensated for) the eventual loss of the word-terminal *alef* — one has to explain why they used *alef* in just this case as far back as we can go with Hebrew and Aramaic, and not the otherwise ubiquitous *hē*’ to spell the word-terminal vowel. In the analogous case of the post-vocalic *alef* used with the postpositive Aramaic determination marker (the so-called definite article), this distinctive ending was helped out in articulation by the glottal stop; the sign is consonantal.⁷

The same must also be said about the numerous West-Semitic person names that end with *alef*. In some instances the consonant could be etymological; in other instances it could be the reflex of an ancient Semitic suffix *-a*, abundantly attested at Ebla, especially in god names, the ancestor, in all likelihood, of the *status determinatus* of Aramaic;⁸ Lipiński explains this *-a* as a marker of “the predicate state of the noun”.⁹ In some instances it seems to be a hypocoristic ending in its own right, its use encouraged by analogy with the proper nouns that inherited the primal *-a*. In all cases, at least in the early stages, when etymological *alef* was still being pronounced at the end of words, the balance of probability is that all *alefs* written at the

² Gogel 1998, p. 65. Strictly speaking these are “historical *spellings*.”

³ Andersen and Freedman 1989, pp. 3-14. The literature on the Tell Fakhariyeh inscription is now so extensive that we simply refer to the entry in Fitzmyer, *et al.* 1992, pp. 36-37.

⁴ Zevit 1980, pp. 22, 29.

⁵ Lipiński 1997, pp. 455-58.

⁶ Gogel 1998, p. 344.

⁷ Lambdin 1971, p. 317, n. 3.

⁸ Kienast 1987.

⁹ Lipiński 1997, p. 48.

end of a word represented the glottal stop, including the secondary introduction of that consonant to safeguard a word-terminal vowel.

Zevit's assertion that *alef* is a *mater lectionis* in אַלֶּף¹⁰ introduced by analogy with אֵלֶּף falls under the same objections. Along with its attestation in Old Aramaic and in Moabite,¹¹ the consistent spelling of this word in North-west Semitic attests that its *alef* represents an ancestral consonant phoneme.¹² The editors of the *editio princeps* of the Tell Fakhariyeh inscription immediately and correctly concluded that *alef* in אַלֶּף was a consonant in the Aramaic text.¹³ Hitherto it had been plausible to infer, from various forms of demonstrative and determinative particles in Semitic languages (in particular Ugaritic *dt*, Arabic *zāt* and Ge'ez *zātti*), that the *alef* in Hebrew אַלֶּף is not etymological. The historical spelling is neutral as evidence for the presence or absence of the glottal stop when the word was said. It is the nature of a historical spelling that it once represented the actual pronunciation and later did not. Its continued use does not permit us to establish the date when its phonetic significance changed. It would not have changed overnight, and it would not have changed everywhere at the same time. When the transition was happening, both pronunciations might have been heard in the same community at the same time, a contrast between careful and sloppy speech.

Statements about the loss of a consonant such as *alef* can be too sweeping. To legitimate them would require every attested *alef* in ancient inscriptions to be assayed. Thus the spelling *אֶלֶּף would be better evidence that the sound of the original phoneme was no longer heard. As already conceded, there is no way for those who identify any *alef* as a *mater lectionis* to know that a reader of that text would not make the sound of the glottal stop at that point; and it is just as impossible for those who think that *alef* was not used as a vowel letter in the early days to demonstrate that it represented only a consonant sound. It seems to be a stand-off. But the balance is not equal. There can be no doubt that the Phoenician alphabet originally made no provision for writing any vowel sound, and it is equally certain that the letter *alef* represented a consonant sound that was part of the ancestral

¹⁰ Zevit 1980, p. 18, note 8.

¹¹ Blau 1980.

¹² Paul Joüon (*Grammaire de l'Hébreu biblique* [Rome: Pontifical Biblical Institute, 1947]) said "probably not" (p. 28), "does not seem to be" (p. 86); but the first statement was dropped from Muraoka's translation, and the second was replaced by "The א of אַלֶּף may be etymological rather than a mere *mater lectionis*" (*A Grammar of Biblical Hebrew*, subsidia biblica 14/1, II. Translation and revision of Joüon 1923 [Rome: Pontifical Biblical Institute, 1991], p. 115).

¹³ Abou Assaf, *et al.* 1982.

Semitic phoneme repertoire. Furthermore, it has long been contended that the use of the vowel letters *waw* and *yod* to represent long back and front vowels respectively was the outcome of historical spelling of monophthongized diphthongs. We no longer think that this is the whole story of the use of those vowel letters.

It has long been suspected that the original impulse for the move from the Phoenician use of strictly consonantal spelling to the selective use of vowel letters came from Aramaic scribes. The evidence from Tell Fakhariyeh shows that the use of *waw* and *yod* to represent vowel sounds, when the spelling is not historical, was much earlier than previously thought. Muraoka has made the plausible suggestion that it was precisely in the circumstances of making a bilingual inscription that Assyrian scribal practice prompted Aramaean scribes to make provision for the writing of vowels. "In particular, scribes who were employed to translate Assyrian texts cannot have failed to notice the nature of the heterogeneous writing system of Akkadian in which vowels were fully represented and long vowels were from time to time represented by their own special symbols".¹⁴

In so far as it is true that the use of vowel letters was not at first the outcome of historical spelling, but a deliberate innovation of the scribes, the retention of otiose letters in historical spellings was following the analogy of already established practice. This shows that the monophthongization of ancestral **aw* and **ay* took place after consonantal spelling came into use. This conclusion modifies the long-held belief that it was the survival of historical spelling of words that contained a long vowel as the result of monophthongization of a diphthong that gave to *waw* and *yod* their new significance as vowel letters. But it is precisely because that development gave to these two letters two possible significations that, when we come across a *waw* or *yod* at a place in a word where it is plausible to believe that there was originally **aw* or **ay*, we don't know whether to read *aw* or *ô* (*ay* or *ê*). It is only when those vowel letters are *not* used in any particular instance that we can be sure that the diphthong has monophthongized and that the resultant long vowel is being spelled defectively, that is, not shown at all. That is more likely to occur in the early stages when scribal practice for Aramaic and Hebrew was still largely dominated by the purely consonantal spelling that remained in place for centuries in the writing of Phoenician. By the same token, it is only when *alef* is not used that we can suspect that the ancestral glottal stop is quiescent. An explicit *alef* could be either a true consonant (ancestral or secondary) or an historical spelling.

¹⁴ Muraoka 1983-84, p. 87.

Alef comes into its own as a *bona fide mater lectionis* only when it is used to spell a vowel as such and the spelling is not historical.¹⁵

Use of the spellings found in early Hebrew inscriptions as evidence of the way words were pronounced can proceed on a sound empirical basis only if one assumes that they wrote it the way they said it — or at least tried to. It is true that conservatism in spelling can perpetuate an *historical* spelling after a consonant has become silent. The consistent use of *hē*’ to spell word-terminal long vowels, other than [ū] and [ī], came into vogue in the earliest stages of the adaptation of the Phoenician alphabet to Aramaic, even though *hē*’ as marker of the f. sg. suffix -â was never a consonant. But, whereas *waw* and *yod* came increasingly into use to spell word-medial long [ū] and [ī] respectively, *hē*’ was never used to spell any word-medial vowel. This skews the system. In any case, whatever the thinking behind this restriction, not all vowel letters used in Aramaic and Hebrew inscriptions originated in historical spellings; phonetic considerations operated from the earliest stages of the use of consonant letters to represent certain vowels. It can still be maintained as a rule that *all* word-terminal vowels were represented by *waw*, *yod*, or *hē*’, never *alef*, and that word-medial ū and ī (rarely other long vowels, notably monophthongized diphthongs) were *sometimes* and increasingly represented by *waw* or *yod*. Occasional scribal lapses are only to be expected, but they are so few that they make no difference to the large picture.

Interpreting “Variable” Spellings of “the Same” Word

The spelling practices described above mean that, if there was no word-terminal vowel letter in the written word, there was no word-terminal vowel in the uttered word. It is, accordingly, bad method that brings chaos into the system to project medieval Masoretic pronunciations back onto ancient Hebrew words, and then to claim that the spelling of some words without vowel letters shows that the rules were not strictly followed. Inferences of this kind are most commonly made with words that end in -ā in Masoretic Hebrew, but which turn up without the expected terminal *hē*’ in the inscriptions. A blatant example of this kind of anachronism is the equating of the adverb ‘t “now” with biblical ‘th, ‘attā (consistently [𐤔 433] — ‘t is attested twice in the Hebrew Bible, and attracts *qere* [Ezek 23:43; Ps 74:6]) and then claiming that this shows that the spelling of the final vowel

¹⁵ “[S]trictly speaking, 𐤀 never attained the status of a *mater lectionis*” (Andersen and Forbes 1986, p. 82), and “never developed in Hebrew as the standard *mater lectionis* for any specific vowel” (p. 85).

was “variable”. Yet the scribes at Lachish and Arad did not vary the spelling of this word; they spelled it consistently **‘t** (𐤔 27),¹⁶ עת rather than עתה. Since we can no longer hear anyone at Arad or Lachish reading their mail, we cannot say dogmatically that they did not enunciate **‘t** as **‘attâ**. But why exempt this one word habitually from the treatment of final long **-â** that was routinely spelled with **hē** in those days? It is simpler to infer that they wrote it the way they said it, and that there was no final vowel on their **‘t**. While the only way to find out for certain how they actually said this word would be to wait until the resurrection and use an Israelite from pre-Exilic times as an informant as we do with speakers of contemporary languages, at the very least the attested spelling עת is most naturally interpreted as a representation of **‘at(t)**. The fact that there are several such word pairs in Hebrew lends plausibility, if not certainty, to that conclusion.

The verb suffix **-tī** “I” is always spelled **-ty** in Masoretic Hebrew when word-terminal. There is no evidence that the vowel of this morpheme was ever lost. It would be perverse to extend the kind of analysis appropriate for **‘t ~ ‘th** to the three known instances in ancient Hebrew inscriptions in which the suffix “I” is spelled simply **-t**, not the expected **-ty** (also attested [Renz 1995, I 284, n. 5]). They are **šlht**. “I sent” (Renz 1995, Mur(7):1,2 [I 283-85]; Davies 1992, 33.001.1); **brkt**. “I blessed” (Renz 1995, KAgr(9):8,1 [I 59-61]; Davies 1992, 8.017.1); **klt** “I finished (weighed?)” (Renz 1995, MHas(7):1,8 [I 315-30]; Davies 1992, 7.001.8; KAI 200;¹⁷ Gibson 1971, 28; Pardee 1982, 21). We quote the word-divider, when one is present, as evidence that the text preserves the complete word. Without becoming overly doctrinaire with the hypothesis that “they wrote it the way they said it” (scribes sometimes make mistakes), the analogous loss of the vowel from **-tī** “thou [2nd f. sg.]” does give a mild reason to suspect that this vowel might have been lost sometimes from the suffix **-tī** “I” in these words. There are three reasonable explanations for these deviations from common practice, with *defective* spelling of a final long vowel, exceptions to the rule that all final vowels were represented by the appropriate vowel letter: (1) scribal carelessness; (2) rare loss of the vowel ending in speech, correctly shown in the writing; (3) the continuing influence of Phoenician orthoepy. In places where Israelite and Phoenician cultures met it would not be surprising if spelling practice was mixed. This is evident in the recently discovered Philistine dedicatory inscription from Eqrn, which, in spite of its

¹⁶ Davies 1992, pp. 467-68.

¹⁷ KAI = *Kanaanäische und aramäische Inschriften*, by H. Donner and W. Röllig (Wiesbaden: Harrassowitz, 1962-64).

late date (seventh century B.C.) and nearness to Israel, does not have a single indubitable vowel letter.¹⁸

While this evidence might require slight relaxation of the claim that all final vowels without exception were represented by an appropriate vowel letter, it does not impeach that induction to the extent that it may be replaced by a rule that “anything goes”.

There are, then, two different explanations of such data. The *consistent* spelling of ת “now” in ancient inscriptions shows that for those writers the word had no final vowel; the *occasional* spelling of the suffix “I” as -t not -ty suggests that a word-final vowel might not always be shown with a vowel letter. The second example can be understood as *lapsus calami* or as a mild perpetuation of Phoenician spelling.

Defective Spelling of Word-terminal Vowels in the Masoretic Text

There is a phenomenon in the Masoretic writing practice in which the vocalization does not match the consonantal orthography, namely the result of the punctuators’ decision to supply *qāmeṣ* to some 3rd sg. f. pronoun suffixes, spelled with consonantal *hē*’ but with no vowel letter and taken to be -hā, contrary to the otherwise universal practice of marking all word-terminal vowels (all of which were long) by an appropriate vowel letter (which would have been *hē*’ in this instance). The same was done to some forms of the 2nd sg. m. suffixes -tā and -kā, and pl. f. -nā, even though they might not have the requisite vowel letter *hē*’, which was used for these suffixes in a small fraction of their occurrences in the received text of the Hebrew Bible (see Table 1). Just how to interpret this evidence is a complicated and much disputed question, which in the context of our present concern takes the form of asking how Hebrew speakers in biblical times pronounced these suffixes. We think it is possible that both forms existed side by side in the classical language,¹⁹ but whether in free fluctuation or as “high style” and “low style” forms we have no way of knowing. The consonantal orthogra-

Table 1. Long and short spellings of pronoun suffixes in the Hebrew Bible							
Spelling	2nd m. sg. (nouns)		2nd m. sg. (verbs)		2nd f. pl. (verbs)		Total
<i>long</i>	כה	38	תה	149	נה	299	486
<i>short</i>	ך	7035	ת	1785	ן	40	8860
Total		7073		1934		339	9346

¹⁸ Andersen 1999.

¹⁹ Birkeland 1955.

phy has first claim, so we take **dbrk**, “thy word”, as reflecting something like ***dabarak** rather than Masoretic **děbārēkā**, **dbryh**, “her words”, as ***dabarayh**, not **děbāreyhā**.

It was not until Lowth’s celebrated lectures that a revolution took place in the attitude of respect for the Jews’ knowledge of their own language. Turning away from the possibility of working out Hebrew poetic metre in terms of the *sounds* of the language, Lowth said:

As to the real quantity, the rhythm, or modulation, these from the present state of the language seem to be altogether unknown, and even to admit of no investigation by human art or industry. It is indeed evident, that the true Hebrew pronunciation is totally lost. The rules concerning it, which were devised by the modern Jews many ages after the language of their ancestors had fallen into disuse, have been long since suspected by the learned to be destitute of authority and truth.... The truth is, it was neither possible for them to recal [sic] the true pronunciation of a language long since obsolete, and to institute afresh the rules of orthoepy; nor can any person in the present age so much as hope to effect any thing to the purpose by the aid of conjecture, in a matter so remote from our senses, and so involved in obscurity.²⁰ [After mentioning the corresponding difficulties with Greek and Latin, he returns to Hebrew.] ...but the state of the Hebrew is far more unfavourable, which, destitute of vowel sounds, has remained altogether silent (if I may use the expression) incapable of utterance upwards of two thousand years. Thus, not so much as the number of syllables, of which each word consisted, could with any certainty be defined, much less the length or quantity of the syllables: and since the regulation of the metre of any language must depend upon two particulars, I mean the number and the length of the syllables, the knowledge of which is utterly unattainable in the Hebrew, he who attempts to restore the true and genuine Hebrew versification, erects an edifice without a foundation.²¹

While Lowth’s remarks were focussed on the problem of scansion of Hebrew verse, the general consequences of his pronouncements in subsequent Old Testament scholarship were far-reaching. Beginning with the work of Lowth himself, notably in his book on Isaiah (the preface repeats the substance of the remarks quoted above), belief that the Masoretic vocalization was worthless gave scholars a license to add vowels to the consonants in any way that suited their purposes.²² There is a grain of truth in this opinion, of course; the vowel points were not supplied until the Middle Ages, and do not possess the same authority as the consonantal text. But it is quite incorrect to infer that, because the vowels were not written, they were not known. The notion is historically absurd that the biblical texts

²⁰ Lowth 1829, p. 44.

²¹ *Ibid.*, p. 45.

²² Norton 1993, II, p. 120.

were “mute”, that the Jews did not know how to say the words written in their Bible so long as the manuscripts contained only consonant letters. On the contrary, there is every reason to believe that there has never been a day in which that text was not read aloud, that knowledge of the correct pronunciation of just about every word was preserved in the oral tradition, and that this knowledge was brilliantly captured and enshrined in the Masoretic points.

Lowth’s skepticism over the work of the Masoretes was unhappily tainted by religious prejudice; he said that “the Jews have been exceedingly deficient” in “critical skill”.²³ Low regard for the work of the punctuators is no longer expressed as stridently as it was by Lowth. Even so, parts of the Tiberian system are still considered to be artificial and arbitrary, at least not corresponding to older traditional pronunciation. Paul Kahle was the most influential scholar in sustaining this attitude. In *The Cairo Geniza* Kahle drew an analogy between the efforts to standardize and stabilize the reading of the Qur’ān²⁴ and the standardization of the pronunciation of the words in the Hebrew Bible. In the former case, “The systematic adaptation of the text of the Qur’ān to Bedouin poetry triumphed over and obliterated the older forms of the Holy Book” (*The Cairo Geniza*, p. 149). In the latter case, particularly in the matter of supplying end-vowels to words that did not have an appropriate vowel letter, “the Masoretes probably followed the example set by the Arab Readers when they introduced end-vowels into the text of the Qur’ān in accordance with Bedouin poetry” (p. 186). The Masoretes also “introduced” (p. 186) two other features into the pronunciation of Hebrew—“a number of new vowels to safeguard the newly-established pronunciation of the gutturals” (p. 186); and “a double pronunciation of the BGDKPT” (p. 186), which might not have existed before the eighth century C.E. (p. 184).

The Historical Value of Masoretic Vocalization

The intentions of the Masoretes should not be misrepresented as if they thought that they were doing strictly historical reconstruction. “The main purpose of their work was still to preserve the *text* of Scripture rather than investigate the rules of the *language* of Scripture”.²⁵ To judge from the results, they were aiming at *consistency*; the resulting uniformity of the

²³ Lowth 1868, p. xlii.

²⁴ Kahle 1959, pp. 141–49.

²⁵ Geoffrey Khan in Horbury 1999, p. 193.

vocalizations in the Masoretic texts is astonishing. The exercise was artificial insofar as they leveled out the historical differences that would have existed in Hebrew pronunciation down through the centuries when the Bible was being composed and edited. And they aimed at *precision*, as manifest in the fine-drawn disagreements over such minutiae as the use of *dagesh*, the provision of *hatef* vowels for some non-laryngeals and the nit-picking disagreements between the Ben Asher and Ben-Naphtali schools. The vowels standardized public reading, and the vowels supplied were those in currency, not the ones used by Moses, or David, or even Ezra.

The validity of Masoretic vocalization has been vigorously defended by Schramm (1964), who argued that the intention of the Masoretes was to record a known traditional pronunciation phonetically. Gordis (1971) pointed out that comparative-historical grammar, placed on a scientific footing since Lowth's day, fully endorses the general credibility of the tradition, especially in the relations between vowels and roots. James Barr spoke against the practice (clearly a legacy from the attitude of Lowth) of respecting the consonantal text while exercising license to emend the vowels, "often implying that it [the vocalization] is a late and ill-informed interpretation which may be modified by scholars at will".²⁶ Barr concludes: "In preference to the sharp distinction between a reliable consonantal text and an unreliable vocalization, one might rather hold that the two aspects were interdependent".²⁷

Morag 1974 argued even more emphatically for the historical validity of Masoretic vocalization, pointing out that some strange forms, suspected by many scholars of being erroneous or merely whimsical guesses to be "emended" to textbook norms, turn out to be precious evidence of authentic dialectal variants when studied more patiently. This does not mean that they are the result of accurate preservation of the forms actually used by biblical authors, only that the unusual vocalizations reflect real developments at some point in the long and complex history of the Hebrew language. In a similar vein Garr 1991 has been able to use fine points in Masoretic vocalizations to trace peculiarities in Hebrew morphophonemics.

All the same, the reliability of the Masoretic vowel points as a true record of actual Hebrew pronunciation should not be overstated. Hughes 1994 has relaxed the position taken by Barr to some degree by pointing out the parts of the grammatical system of the Hebrew language where archaic forms and ancient usage were no longer recognized by later readers. Instances are the reading of an ancient infinitive absolute as a perfect verb, or the misreading

²⁶ Barr 1968, p. 188.

²⁷ *Ibid.*, p. 218.

of a forgotten archaic imperfect *qal* passive as *niphal*. The reversal of such updating moves on the part of later scribes would be a hazardous undertaking for us now. But such slippage in the identity of some words, at least in the details of their pronunciation, is a warning against assuming too much similarity between Masoretic and inscriptional Hebrew.

The Mixed Evidence for the Pronominal Ending -ā

It needs to be emphasized that the phenomena on which Kahle based his arguments (that the vocalization was contrived and had no correspondence with actual pronunciations in ancient times) consist of only three of the numerous features of the Tiberian system. What of the majority of vowels? It is true also that the existence of other systems of vocalization, notably Babylonian and Palestinian — and Kahle did more than any one to recover them — shows that pronunciation varied in different parts of the Jewish community in medieval times, as to this day.

Of the three pieces of Tiberian vocalization that Kahle thought were artificial, only one — the equivocal status of Masoretic terminal *-ā* on pronoun suffixes — is relevant for reconstructing the early historical stages of Hebrew morphology. In this regard, the evidence of the Dead Sea Scrolls, along with the evidence of the *Secunda* of Origen's *Hexapla*, shows that some adjustments to Masoretic word shapes were relatively late. For our purposes we can use a general date for the evidence of the *Secunda*; we do not have to decide between the view that Origen himself did the transcription and the view that he utilized a version of the Hebrew scriptures in Greek letters that had been in use before his time. Ben-Ḥayyim wrote a monograph on this subject in which he said:

No new attempt to find a solution for the problems of pronunciation and accentuation bound up with the suffix *-kā* and others similar to it will be of any advantage as long as the degree of reliability to be attached to the Tiberian tradition in this matter and what may be learnt from other, external traditions is not made absolutely clear.²⁸

The problem is illustrated by the two attested spellings of a pronoun such as “them” [m.] **hm** or **hmh**, or a pronoun suffix such as “thou” [m.] **-k** or **-kh**. The scribes treated such phenomena in three different ways, inconsistent among themselves. With the free forms, the Masoretes read and vocalized the two spellings in accordance with the almost universal rules of Hebrew orthography. The terminal *hē*’ was a vowel letter; the two

²⁸ Ben-Ḥayyim 1954, p. 21.

forms represented two forms of the pronoun — **hēm** and **hēmmâ**, both current in the language.²⁹ In the instances of suffixed pronoun forms, by contrast, the Masoretic vocalization encoded only one pronunciation, in spite of the two spellings. In the case of the (rare) spelling of “thy” [f.] as **-ky** rather than the usual **-k**, and of “thou” [f.] as **-ty** rather than the usual **-t** — the longer forms being genuine archaisms — they followed the majority spelling (no vowel letter, so no terminal vowel) and “corrected” the *plene* spellings with the help of *qere* readings, normalized by dropping the *yod*. It is to their credit that they did not delete the otiose *yod* from the text. When the vowel letter was **hēʾ**, however, they took the opposite view. The vocalization followed the minority spelling (vowel letter, so there was a terminal vowel when that spelling was used) — 486 out of 9346 instances or 5.2 percent (see Table 1). But by leveling the vocalization of the 8860 suffixes written without **hēʾ**, assuming that there was a terminal vowel even when no vowel letter indicated it — a practice the opposite of their treatment of **hēmmâ** and **hēm** — they made the pronunciation of the three suffixes presented in Table 1 exceptions to the otherwise universal rule in Biblical Hebrew that a terminal vowel is always spelled with the appropriate vowel letter.

Kahle’s explanations of the Masoretic vocalization of such pronoun suffixes were not consistent. On the one hand, he asserted that the forms were “innovations” (“made in the eighth century” [*The Cairo Geniza*, p. 175]) imitating the poetic style with case endings preferred for the Qurʾān, which the Jewish sages used as a “model” (p. 177). Kahle supported his claim with arguments developed by Sievers, that the long endings were not authentic because they did not fit the anapestic rhythms that Sievers believed prevailed in Hebrew verse (pp. 175-76). This line of argumentation no longer cuts any ice. On the other hand the abundant occurrence in the Qumran manuscripts of the longer spellings with **hēʾ**, which could only be read as a marker for **-â**, proved beyond all cavil that the forms were real in earlier Hebrew. But how extensively these longer endings were used in biblical times was not clear. Kahle conceded that “the a-vowel must sometimes have been pronounced in earlier times” (p. 176), but asserted that it “completely disappeared” in later times (p. 176) and “was lost for centuries” (p. 175). In support of this claim he adduced the evidence of the *Secunda*. But the evidence of the *Secunda* is mixed; both forms are attested (Janssens 1982). In words that Masoretic vocalization normalized to **-kā** *Secunda* has **-αχ** twenty-two times, **-αχ**α once with nouns; **-εχ** five times, **-εχ**α once with

²⁹ Freedman, Forbes and Andersen 1992, p. 62.

verbs (Janssens 1982, p. 87). Kahle dismissed these longer forms in the *Secunda* as “exceptions” (*Cairo Geniza*, p. 171). But the mixed evidence of the *Secunda* agrees with the mixed evidence of the consonantal orthography of the traditional text, and with the use of both short and long spellings at Qumran.

Ben-Ḥayyīm 1954 assembled an extensive listing of the relevant data from medieval manuscripts, biblical and non-biblical. This evidence can be summarized as follows. (1) The suffix “thy” [m.] is consistently **-kā** in all biblical texts (whether the punctuation is Babylonian, Palestinian, or Tiberian), no matter what the consonantal spelling. (2) In Greek and Latin transcriptions of Hebrew words bearing this suffix, it is mainly **-ak** (¥ 50), otherwise **-ka** (¥ 5). (3) In non-biblical Hebrew texts with Palestinian punctuation “the suffix **-k** is always without a final vowel” (Ben-Ḥayyīm 1954, p. 31). (4) In non-biblical Hebrew texts with Babylonian punctuation a fairly consistent distinction is made between words with **-k** whose stem ends in a consonant (the suffix is **-āk**) and those whose stem ends in a vowel (the suffix is **-kā**). (5) In the Samaritan tradition (biblical and non-biblical texts) the suffix **-k** never has a final vowel (Ben-Ḥayyīm 1954, p. 37).

In contrast to Kahle’s explanation — that the short pronunciation prevailed in Hebrew (being correctly represented by the non-use of *hē*) until the longer pronunciation was artificially imported by imitation of Arabic — Ben-Ḥayyīm argued that the long pronunciation was the only correct Hebrew (“handed down from ancient times” [Ben-Ḥayyīm 1954, p. 63] and correctly represented in the Masoretic vocalization) and that the spurious vowel-less pronunciation, so abundantly attested in Greek and Latin transcriptions, in the Samaritan tradition, and in non-biblical Jewish Hebrew compositions, was due to “the pronunciation of Hebrew after the fashion of Aramaic” (Ben-Ḥayyīm 1954, p. 59). However, he missed the crucial point; if the long pronunciation was the correct one for Biblical Hebrew, why was it not represented in the consonantal orthography, why was the spelling of this one suffix so completely out of line with the otherwise consistent scribal practice?

Similar arguments have been advanced to account for various peculiarities in the Hebrew of the Dead Sea Scrolls as Aramaizing or perhaps in some cases as orthographic means of resisting Aramaizing tendencies. The greater frequency of the **-kh** spelling of our suffix in Qumran texts would then be explained as securing the Hebrew long form and discouraging the Aramaic short form. Ben-Ḥayyīm does not address the problem of why this useful *hē* was not utilized more for the same purpose in mainstream biblical texts.

It is better to explain the phenomena as evidence of the existence and persistence of *both* forms of this suffix (as well as of others like it) *within Hebrew*, without bringing Aramaic or Arabic into it. While one community might prefer one pronunciation or the other (and the Masoretes preferred and canonized the longer form), it seems as if the Qumran community was comfortable with both forms. Perhaps the most striking indication of the currency of both forms in Qumran spoken Hebrew is the survival of two copies of the same prayer, each consistent in this very detail: **bkwhk... lk... brytk... ydyk** (1Q34^{bis} 3 i 4-8); **bkwhkh... [l]kh... [bry]tkh... ydykh** (4Q509 97-98 i 3-12).

If the orthographic evidence for the coexistence of long and short forms is to be taken seriously — the alternative is the anarchistic view that both forms of the pronominal elements could be spelled either way at any time, regardless of the pronunciation that they were supposed to be representing — then it indicates that both forms existed side by side in Hebrew at various times.³⁰ Goshen-Gottstein, among others, drawing attention to other similar pronominal pairs, argued that their undoubted coexistence in the biblical text suggests “that at some point in the history of Hebrew two alternative paradigms came into existence through analogy, patterned on the types -C /-C(C)a, only part of which is shown by MT”.³¹ If they were not simply in free fluctuation, the distribution of usage of these two available forms for some pronominal suffixes and free pronouns remains obscure. Brekelmans raises a methodological issue: “Should we accept that in one dialect all the different forms that existed were used at the same time and by the same author?”³²

Clues from other Semitic Languages

The assistance that comparative-historical grammar might bring to the resolution of the question of the vowel endings on pronoun suffixes and verb affixes works two ways. It is positive insofar as it shows that longer forms match ancestral forms (see Appendix); but that assistance is negative when the reconstruction of the history of the suffixes supposes that the long forms were lost in an intermediate phase and recovered in a later phase. Theoretical objections have been raised against the likelihood of such a process.³³ Kahle explained the rehabilitation of the long-lost vowel endings

³⁰ See the Excursus in Cross and Freedman 1952, pp. 65-70.

³¹ Goshen-Gottstein 1960, pp. 114-15. See also Freedman, Forbes, and Andersen 1992, pp. 61-63.

³² Brekelmans 1969, p. 8, n. 9.

³³ Steiner 1979.

of pronoun suffixes through imitation of Arabic, but also suggested that the Masoretes, whom he supposes to have been Karaites, had come into possession of old biblical manuscripts in which the spellings with *hē* were used, as at Qumran and in contrast to the texts that had come down through rabbinic channels (*Cairo Geniza*, p. 80-82; 177; 186). The longer pronunciation suggested by these old spellings happened to coincide with the result of imitating readings of the Qurʾān. This complicated scenario is not needed. Birkeland criticized Kahle's position, asking, "Why does he not use the simpler explanation of a double stream of linguistic development?"³⁴

In the case of the similar phenomenon in Aramaic, objection to the theory of loss and later recovery of terminal vowels has led Cook to argue that, since the vowels are attested late, and could not have been lost and recovered, they must have been there all the time, but spelled defectively.³⁵ Scagliarini 1990 has discussed similar problems. Kutscher 1957 and Blau 1982 have argued for a process in which a more archaic form remained in continuous use in one dialect and was later brought back into a dialect that had lost it in the interim.

We seem then to have theoretical or methodological problems of equal difficulty. Objections have been raised to the explanation that the older ending was lost and then later rehabilitated; likewise to the theory that such variant pairs continued to exist side by side in the same language; and likewise the postulate that the variants must have existed in geographically isolated dialects, such as Eastern and Western branches in the case of Aramaic (Blau 1982; Kutscher 1957), or that this pattern spilled over from Aramaic (or Arabic [Kahle 1959]) into Hebrew.

Hebrew Dialects

A more nuanced understanding of historical dialectology recognizes variant forms could coexist in one language and be distinguished sociolinguistically. Along such lines Cross and Freedman already suggested, "The longer form of the suffix was native to old Hebrew, and survived in elevated speech and literary works. The shorter form developed in the popular speech at a very early date".³⁶ This argument, although somewhat *a priori*, is cogent. It recognizes that more than one pronunciation of "the same" entity can coexist in a language, if not completely interchangeably, at least as having preferential vogue in distinct social registers. In historical per-

³⁴ Birkeland 1955, p. 27.

³⁵ Cook 1990.

³⁶ Cross and Freedman 1952, p. 66.

spective, if the terminal vowel represents the survival of an original short -a by [abnormal?] lengthening (Steiner 1979; Blau 1982), then it is archaic, and might be expected to be preferred as the “literary” (“versus the vulgar”) form.³⁷ Birkeland’s explanation of this “double stream” was that the Hebrews reacted against the Canaanite tendency towards elision of final short vowels, trying to retain the short final vowels of their “desert language”³⁸ by lengthening them, adding that “the reaction must have been especially strong in the higher, representative classes, relatively weak in the lower classes”. (But in things like this the less sophisticated strata of society are often more conservative in their language habits.) Murtonen, to the contrary, considered that the short form was “the more solemn form of recitation which alone represented a natural development according to the rules of Hebrew grammar”,³⁹ as against the use of the longer form in “every-day speech”. The question of regional and social dialects of Hebrew throughout its entire history is a valid one. Unfortunately, research has not yet succeeded in mapping these dialects. The recent monograph of Young 1993 has freshly formulated real questions along these lines.

Neither Cross and Freedman nor Birkeland, nor Murtonen supported their (quite different) positions on the variant long and short pronoun endings with a detailed distributional analysis of the evidence. There is no point in such an exercise unless orthography as such is taken seriously. Spelling reflects pronunciation. The epigraphic evidence attests consistent use of vowel letters in early North-west Semitic alphabetic writing for all terminal vowels. Occasional misspellings have to be allowed for; but the accidental omission of the required vowel letter is not evidence that either spelling was acceptable in general practice. Long -â at the end of any word was represented by *hē*’. Now, in regard to the pronominal elements under discussion, if most of the Hebrew Bible was written in “literary”, versus “popular”, not to say “vulgar”, Hebrew (if “elevated speech” as in cult poetry or epic history preferred archaic forms), then mostly -kâ, -tâ, and -nâ would have been used, and the spelling with *hē*’ would have predominated. It is quite the opposite in the text of the Hebrew Bible we now have. The shorter spellings prevail (Table 1).

The orthographic peculiarities of the Masoretic Text in its representation of these suffixes are hard to explain. (1) Assuming that the longer ending was preferred in elevated speech and in literature, the universal practice of writing -â with *hē*’ would have resulted in the dominant presence of this

³⁷ Cross 1970, p. 305, n. 11.

³⁸ Birkeland 1955, p. 27.

³⁹ In Kahle 1959, p. 176.

spelling in biblical texts. The extant low incidence of this spelling in the texts from which the Masoretic tradition has come down would then be the result of the later triumph of the shorter pronunciation in that reading tradition, a pronunciation inconsistent with the presence of the *hē* spelling, so that the deletion of that now redundant *hē* would have been needed to secure the “correct” short pronunciation. While theoretically conceivable, this complicated scenario is quite unlikely. Once the stylish pronunciation of those suffixes with -â had been put into place in written texts by the use of *hē*, the presence of this vowel letter would have maintained that pronunciation in the reading of sacred texts, even after it had become obsolete in ordinary speech. The usual conservatism of scribes makes it hard to believe that at some stage in the history of biblical texts more than ten thousand *hē*’s had been deleted to make the spelling match the shorter pronunciation (nearly nine thousand as in Table 1 and more than 1,300 “her” הָר not *הָרִי). Even so, there was systematic and wholesale adjustment of certain classes of spelling, notably the replacement of *hē* by *waw* to spell word-terminal -ô, and the adding of a (silent!) *yod* to -āw, “his”, on plural noun stems, apparently a purely scribal marker with no phonetic value. One can see the reason for this; but no similar considerations would explain the wholesale *deletion* of superfluous *hē*’s, especially if the forms with the additional -â were still current as acceptable alternate pronunciations.

(2) It is even more unlikely that the ending -â was pronounced, but not represented by *hē* in the manuscript sources used by the rabbis. There is no reason why just those vowel endings, on just those few word types, should have been singled out for exemption from the otherwise universal practice of representing word-terminal vowels by means of vowel letters. (In the analogous case of Aramaic, Cook 1990 argued that the -a of -tā, “thou” [m.] was spelled defectively because it was not stressed, while admitting that the matching -î of -tî [f.], also unstressed, was written with *yod*. No reason was given for this inconsistent practice.) If, however, for no reason that we can guess, the vowel letter *hē* was not used for this terminal vowel in the mainstream of manuscript transmission, the differing practice of the Qumran scribes, who used *hē* generously for these kinds of suffixes, even in biblical texts and most conspicuously in the great Isaiah scroll (1QIsa^a), would represent a belated provision of the correct spelling. It is more likely that this striking difference in the spelling practices of the Qumran scribes, not only for these particular vowels, but in line with their use of consonant letters to spell all long vowels, and even some short ones, is the outcome of a conscious policy of more plenary *phonetic* spelling (perhaps to keep Hebrew pronunciation distinct from Aramaic). In their community the longer pro-

nunciation of these suffixes had come to the fore (we do not know how or why), and was used in later times by the larger Jewish community in the reading of their scriptures, even when there was no vowel letter to prompt the longer pronunciation. The later Masoretic vocalization finally enshrined this practice by using *qameṣ*, but they did not make the concomitant change in the consonantal text. They did not feel free to add the appropriate vowel letter *hē*, even though this left a glaring inconsistency in Masoretic orthoepy.

(3) In the Masoretic Text nearly ninety-five percent of the suffixes -*k(â)*, -*t(â)*, -*n(â)* are spelled defectively. The picture is similar for the 3rd f. sg. suffix, -*hâ* in Masoretic pointing. If this pronunciation had been in vogue in biblical times, the spelling would have been -*hh*. It is not valid to argue, as Blau 1976 (p. 8) has done, that they did not like to use a vowel letter after the same letter used as a consonant. In all cases (apart from this suffix) where a word ends in -*hâ*, it is spelled -*hh*. But of more than 1300 occurrences of -*hâ*, “her”, in the Hebrew Bible, only one (Ezek 41:15—a lonely intrusion of postbiblical usage) is spelled -*h’*, none -*hh*.⁴⁰ In other words, the consonantal Masoretic Text retains no clear evidence that the longer forms of these pronominal elements were distinctively literary, or even that the alloform -*hâ* of the 3rd f. sg. suffix existed at all in Biblical Hebrew. The least complicated explanation of these data is that the terminal vowels of these suffixes were not written because they were not pronounced when the Proto-Masoretic manuscripts acquired some kind of official recognition. The consistent orthography points to the domination of the mainstream Biblical Hebrew by the shorter (vowelless) forms of these suffixes. The domination of biblical texts by the shorter (vowelless) pronunciation makes it difficult to maintain that this was the “vulgar” dialectal variant. It was standard. It is more likely that Qumran scribes added *hē* because they used the longer -*â* pronunciation. Their mixed praxis suggests that they found both pronunciations acceptable. This evidence, along with the fact that the longer forms are attested in biblical texts, albeit sparingly, is enough to show that both forms are genuine.

Hebraists have no difficulty with the occurrence in the language of such pairs as *hēm* and *hēmmâ*. But no distribution patterns have been discerned for any of these pairs.⁴¹ It has to be admitted that, in spite of a number of partial studies of ancient Hebrew dialectology (Garr 1985; Rendsburg 1980, 1982; Young 1993), variants of the kind met with in the pronoun suffixes

⁴⁰ On the general problem of the vocalization of words that end with **נָחַם** see Freedman, Forbes, and Andersen 1992, pp. 245-49.

⁴¹ *Ibid.*, p. 62.

and attested in the Hebrew Bible have not been mapped onto dialects, whether regional or social.

The attestation of the two pronunciations of the free form of the 2nd m. sg. pronoun “thou” is similar: אַתָּ 5 א , אַתָּה 744 א . The Masoretes read them all as the same — ’attā, and the orthography shows that longer ’attā was almost universally preferred for this particular word, at least in the estimation of the scribes responsible for establishing the canonical text. The situation is all the more striking and unimpeachable because it is the mirror image of the situation with the suffixed allomorphs of this pronoun; neither was leveled to the other. At the same time the occurrence of the shorter spelling shows that the pronunciation ’att also occurred. Being so rare, it is hardly likely that ’att was the more respectable form of this pronoun. The five instances of אַתָּ , “thou”, are in 1 Sam 24:19; Ps 6:4; Job 1:10; Qoh 7:22; Neh 9:6. There are none in the Pentateuch. Literature and perhaps the speech of the upper classes normally used the longer form of this free pronoun, but in the case of the corresponding suffix -k it seems to have been the other way around. They preferred the short form of the suffix.

The distribution of the 486 longer forms (going by spelling) of the three pronoun suffixes that also had a short pronunciation (if that is the significance of the *defective* spelling) (Table 2) does not show any marked concentration in any portion of the canon that would point to preferential use of the more archaic longer form in literary compositions in contrast to the supposed preference for the shorter form in lower class speech.

The concentration of longer endings in the Torah contrasts with their rarity in the Writings, suggesting that the forms are archaic, but nowhere do longer forms account for more than a small fraction of the total. Their sparse occurrence in the poetic books weakens the argument that the forms are “elevated” (unless one supposes that they were there once but were cleaned out later).

Table 2. Distribution of the longer pronoun suffixes				
Portion	-kâ	-tâ	-nâ	Total
Torah	18	44	46	108
Former Prophets	8	41	32	81
Latter Prophets	6	22	144	172
Poetry (Ps, Jb, Pr, La)	6	30	40	76
Other Writings	—	12	37	49
Total	38	149	299	486

The term “variable” might be appropriate for the spelling of word-internal long vowels; but the practice was not arbitrary.⁴² There were constraints and trends to greater use of internal vowel letters with the passage of time. Thus already by the end of the eighth century BCE the “Royal Steward” Silwan Tomb Inscription has **’rwr**, the first known instance of such a spelling. But the marginal availability of this option for some word-medial long vowels does not mean that a similar freedom was exercised in the representation of word-final vowels.

Second Masculine Singular Pronouns in the Hebrew Bible and in the Inscriptions

The story of the various forms of the 2nd m. sg. personal pronoun forms is nuanced. There are three morphemes, all attested in a longer and a shorter form, both in the Hebrew Bible and in the inscriptions: (1) the free form **’t** ~ **’th**; (2) the verb-subject suffix **-t** ~ **-th**; (3) the suffix **-k** ~ **-kh** attached to verbs (as object), prepositions, and nouns. The attestation of these alloforms by consonantal spelling in the Hebrew Bible is skewed; the longer form is preferred for the free pronoun, the shorter form for suffixes, as shown above. This asymmetry is rather surprising, and it shows that each of the forms goes its own way. They have different distribution patterns. The short free pronoun (0.67%) and the long form **-kh** (0.53%) are very rare in the Hebrew Bible, the verbal **-th** (7.7%) is more frequent on the average, but still rare. We do not know the basis of Beit-Arieh’s assertion that the longer form appears 111 times in the Bible (our count is ¥149). But it is misleading to say that it “occurs regularly”.⁴³ According to the dictionary, “regular” is customary, usual, following a set rule or normal practice. An option taken in fewer than eight percent of opportunities is not made “regularly”, not even frequently, only occasionally.

The evidence from inscriptions is similarly mixed. Proportionately more longer forms are attested in the inscriptions than in the Hebrew Bible

⁴² The term “variable” drives James Barr’s quite unscientific treatment of the orthography of biblical Hebrew in Barr 1989. The term would be innocuous if it only had the trivial reference to the fact that a word that is spelled acceptably in more than one way has “variable” spelling. But Barr infers that variable spellings in the Hebrew Bible are arbitrary, even random. He makes no attempt to demonstrate randomness using familiar statistical metrics; indeed he openly despises “arithmetic.” See my review in Andersen 1991, pp. 121–26. That many variable spellings are *not* random has been rigorously proved by Andersen and Forbes 1986, but although some biblical scholars who lack the appropriate scientific training have sneered at this work, not a single line has yet appeared in the literature purporting to impugn its methods or refute its results.

⁴³ Beit-Arieh 1993, p. 63.

(Table 3). But the counts are so small that any statistical inference to general usage would be precarious.

Table 3: Short and Long Forms of Second Masculine Singular Personal Pronouns

Form	Masoretic Text	Inscriptions
ʾt	5	2
ʾth	744	1(?)
-t	1785	7(?)
-th	149	8 (perhaps 10)
-k	7076	73
-kh	38	1

It is precisely because the representation of word-final \bar{a} with $h\bar{e}'$ is so ancient, so abundant, and so invariable in all other areas of Hebrew spelling that we interpret the absence of $h\bar{e}'$ at the end of a word as an indication that there was no vowel at the end of that word. We have already maintained this inference in the case of ʾt “now” in the inscriptions. In the case of the 2nd sg. m. pronouns, the evidence of the consonantal text of the Hebrew Bible (Table 3) shows that the free form was usually long ʾattā, but could be ʾat(t), while the suffixes were mainly -(a)t and -(a)k, but occasionally -tā and -kā.

While the evidence from the inscriptions is sparse compared with the Hebrew Bible (fewer than a hundred specimens versus nearly ten thousand), it presents a similar picture. Subject to correction and, of course, open to revision as new inscriptions come to light, the following inventory is complete for Davies 1992 and Renz 1995, augmented by the Ḥorvat ʾUza ostrakon published in *Tel Aviv* 20 (1993) and the Moussaïeff ostrakon (Bordreuil, Israel, and Pardee 1996, 1998).⁴⁴

I. Verbs with -t

ʾm[rt “thou didst say” — *AI*⁴⁵ 40; Davies 1992, 2.040.5; Renz *Arad*(8):40 (I 147); *IR*⁴⁶ #62; Lemaire 1977, pp. 207-209; Pardee 1982, pp. 63-65. The restoration is conjectural.

⁴⁴ Doubt has been expressed over the authenticity of these ostraca by Berlejung and Schüle 1998 and by Eph'al and Naveh 1998.

⁴⁵ *AI* = Yohanan Aharoni, *Arad Inscriptions* (Jerusalem: The Israel Exploration Society, 1981).

⁴⁶ *IR* = *Inscriptions Reveal: Documents from the time of the Bible, the Mishna and the Talmud*. Miriam Tadmor, ed. (Jerusalem: Israel Museum).

- wlqh̄t** “and thou shalt take” (the injunctive mood is supplied by discourse coherence with the preceding imperative **spr**) — *AI* 3; Davies 1992, 2.003.8; Renz Arad(6):3 (I 363); *IR* #52; Lemaire 1977, pp. 163-66; Pardee 1982, pp. 34-38.
- wlqh̄t**. “and thou shalt take” (the injunctive mood is supplied by coherence with the preceding imperative **b’**) — *AI* 17; Davies 1992, 2.017.3|4; Renz Arad(6):17 (I 381); *IR* #54; Gibson 1971, p. 53 D; Lemaire 1977, pp. 174-79; Pardee 1982, pp. 51-53. Freedman 1969 rightly maintained that this spelling showed that there was no terminal vowel, as against the practice of assuming Masoretic vocalization and therefore nonuse of a vowel letter for a word-terminal vowel.
- wnt|t**. “and thou shalt give” (quasi imperative after a conditional subordinate clause) — *AI* 2; Davies 1992, 2.002.7|8; Renz Arad(6):2 (I 360); *IR* #50; Lemaire 1977, pp. 161-63; Pardee 1982, pp. 33-34.
- wh|sbt** “make (them) do the rounds” (the injunctive mood is supplied by coherence with the preceding imperative **ntn**, which could be infinitive) — *AI* 2; Davies 1992, 2.002.5|6; Renz Arad(6):2 (I 359); *IR* #50; Lemaire 1977, pp. 161-63; Pardee 1982, pp. 33-34.
- wšrr̄t** “and thou shalt bind” (the injunctive mood is supplied by coherence with the preceding imperative **tn**) — *AI* 3; Davies 1992, 2.003.5; Renz Arad(6):3 (I 363); *IR* #52; Lemaire 1977, pp. 163-66 (he reads the final letter as a word divider, so that the form is imperative, and translates “et *harnache-les avec un harnais*”); Pardee 1982, pp. 34-38.
- [šl]̄h̄t “thou hast sent” — *LL*⁴⁷ 5; Davies 1992, 1.005.4; Renz Lak(6):1.5 (I 424); *KAI* 195; Gibson 1971, p. 43; Lemaire 1977, pp. 117-20; Pardee 1982, pp. 95-98. The long-running controversy over the reading of this word is reflected in the range of transcriptions supplied in the literature. Renz 𐤔𐤌𐤔𐤌 [w].

Of these seven instances, five are clearly -t forms.

II. Verbs with -th

- dbρθ** “thou didst say” — Moussaïeff ostrakon (l. 5).
- yd̄th** “thou dost know” — *LL* 2; Davies 1992, 1.002.6; Renz Lak(6):1.2 (I 412); *KAI* 192; Gibson 1971, p. 37; Lemaire 1977, pp. 97-100; Pardee 1982, pp. 78-81.
- yd̄th**. “thou dost know” — *LL* 3; Davies 1992, 1.003.8; Renz Lak(6):1.3 (I 417); *IR* #77; *KAI* 193; Gibson 1971, p. 38; Lemaire 1977, pp. 100-109; Pardee 1982, pp. 81-89.

⁴⁷ *LL* = *Lachish Letters*.

yď^oth “thou dost know” — *AI* 40; Davies 1992, 2.040.9; Renz Arad(8):40 (I 147); *IR* #62; Lemaire 1977, pp. 207-209; Pardee 1982, pp. 63-65.
[w]ktbth “and thou didst write” — *AI* 7; Davies 1992 2.007.5[6]; Renz Arad(6):7 (I 368); Lemaire 1977, pp. 168-69; Pardee 1982, pp. 40-41.
wntth “and do thou give” — Moussaïeff ostrakon (l. 4).
wntth “thou hast given” — Moussaïeff ostrakon (l. 7).
šlh^oth “thou hast sent” — *LL* 3; Davies 1992, 1.003.6; Renz Lak(6):1.3 (I 417); *IR* #77; *KAI* 193; Gibson 1971, p. 38; Lemaire 1977, pp. 100-109; Pardee 1982, pp. 81-89.
šlh^oth “thou hast sent” — *LL* 4; Davies 1992, 1.004.4; Renz Lak(6):1.4 (I 421); *IR* #78; *KAI* 194; Gibson 1971, p. 41; Lemaire 1977, pp. 110-117; Pardee 1982, pp. 89-95.

In both *LL* 3 and *LL* 4 the readings above are uncertain, and an alternative **šlh** **’dny** l **’bdk** is favored by the presence of the expressions **’šr. šlh** **’dny** (Davies 1992, 1.004.2; 1.018.3), **’šr ntn** | **’dny** (Davies 1992, 1.004.11[12]), and **ky šlh** **’dny** (Davies, 1992 1.006.3). Pardee (1982, p. 94) adopts that reading for *LL* 3, but not for *LL* 4 (p. 91). The deferential “my lord sent” rather than “thou didst send” is preferred; **’dny** is attested eighteen times in the Arad inscriptions.

whtr^orth “you shall be destroyed” (Beit-Arieh 1993, p. 58); “you shall be stripped naked” (Cross in Beit Azien 1993, p. 64) — Ḥorvat ‘Uza ostrakon (line 9).

There are, then, five certain attestations of the longer **-th** suffix, with two more possible and doubt hanging over the three supplied by the Moussaïeff ostrakon. Beit-Arieh (1993, p. 61) cited the four instances in *LL* 3, 4 and *AI* 17, 40 (Beit-Arieh 1993, p. 61). We have been unable to confirm his claim that this spelling is also attested in a Khirbet el-Qom inscription.

III. Suffixes with **-k**

This is the standard spelling in the inscriptions, with instances so numerous (we have counted 73) that there is no need to list them.

IV. Suffixes with **-kh**

As in the Hebrew Bible (about one half of one percent) this is rarely attested in the inscriptions. We know of only one certain instance — **qbrkh** ‘thy grave’ in line 13 of the Ḥorvat ‘Uza ostrakon. Four other instances of this suffix in that inscription are read by Beit-Arieh as **-k** — **yšqlk** (l. 5), **mtyk** (l. 7), **šbtk** (l. 7), **’llk** (l. 8).

As with an earlier study of the Khirbet Beit Lei Burial Cave Inscription A (Davies 1992, 15.006; Renz Blay (7):1; Gibson 1971, 58A; *IR* 79), Frank Moore Cross proceeded to his decipherment and interpretation with three assumptions: (1) the text was literary; (2) literature used a higher register than the vernacular, with a preference for archaic alternatives; (3) the longer form of the 2nd m. sg. suffix would have been used in such a text. Cross 1970 read the first line as [']¹n' y. yhw^hh [.] 'lhykh. 'rsh All other scholars who have studied the text independently read yhw^hh [.] 'lhy kl h'rš (Renz 1995, p. 245). The grafitto is admittedly very problematical from every point of view. But it is misleading to quote Cross's reading (P. D. Miller in *Vienna Congress Volume VTSup* 32 (1981): 320-23; *TWOT* 5: 504) without recognizing its tentative nature. It cannot be accepted as a proven instance of the spelling -kh. In similar fashion Cross found more specimens in the Ḥorvat 'Uza ostrakon — yšqlk [h (l. 5), šbtkh[(l. 7), pn[ykh] (l. 9). Cross read Beit-Arieh's 'llk (l. 8). as 'llt. Final assessment must wait until the definitive publication appears.⁴⁸ In the meantime qbrkh in the last line of the Ḥorvat 'Uza ostrakon is the only certain instance of the spelling -kh that we know of in ancient Hebrew inscriptions. Hence the score of "1" in the above table.

This inscriptional evidence shows that the shortened form -k was already in general use in pre-exilic times, at least at the everyday level, in contrast with the longer verbal suffix -th, which was commonly used. The Moussai-eff ostrakon presents striking evidence of this skewed distribution, but the cloud that hangs over its authenticity weakens the point. All three verbs have -th; all four nouns have -k. This contrast is contrary to the intuition that analogy would keep these two closely related suffixes in line. A similar pattern of complementary distribution of -th and -k has been known for a long time from the Lachish Letters, but not enough attention has been given to it. Thus *LL* 3 has nine instances of -k (never -kh) and one of -th. The spelling was consistent, not variable. The straight-forward explanation is that the suffix "thy" was spelled -k because there was no terminal vowel, and the verb subject "thee" was spelled -th because it had a terminal vowel in the habits of those people. With over seventy instances of -k known, the prevailing pronunciation did not have a final vowel. The old Hebrew alphabet was phonetic. If the way people wrote words was intended to tell a reader how to say them, then the orthography of the inscriptions themselves must prevail over *a priori* arguments based on genetic reconstruction

⁴⁸ I am grateful to Professor Beit-Arieh for sending me his working photograph of this inscription. Perusal did not permit the issue to be resolved.

(the suffix was *-ka* in ancestral Hebrew) and over *a posteriori* arguments that project medieval pronunciation (Masoretic *-kā*) back one thousand years.

It could be objected that none of the epigraphic texts was “literary”, so they do not prove that the longer form was not still used in the higher registers of the language. But the Hebrew Bible is now almost the only evidence we have for the use of the Hebrew language in higher culture, and in the matter under investigation it does not differ significantly from the inscriptions. In fact their general agreement in this matter provides mutual support of the authenticity of the inscriptions and the antiquity of the biblical texts. The one exception — and it is a significant one — is the discovery of silver amulets bearing copies of the priestly benediction (Num 6:24-26).⁴⁹ It is pertinent, because the text contains the suffix we are studying. The date has not been settled, but has been reasonably set to round about the sixth century B.C.E. Its testimony is unequivocal. The spelling is consistently *-k*. We would expect such a sacred and venerable text to preserve the archaic forms supposedly of the high register; but it doesn't.

Appendix: The Primal Form of the Semitic Second Person Singular Masculine Personal Pronoun (Free Form)

The several components of the free form of the 2nd m. sg. personal pronoun “thou” can be reconstructed with different degrees of certainty. Everything points to the stem **ant-*. There is a possibility of **ank-*, at least in ancestral Afrasian (Diakonoff 1967, p. 218; 1988, p. 71), taking into account the fluctuation of *k* and *t* among the persons within the pronominal system, with paradigmatic analogy working in either direction. So far as Semitic is concerned, however, the evidence is solidly for **ant-*. The final vowel is well attested as *-a*, with some possibility that **-u* lies behind it (Gelb 1969, p. 178). In Eblaite written *an-da* (Gelb 1981, p. 25). So far as NW Semitic is concerned, the evidence is unequivocal for **att-*. For Canaanite the evidence of the Amarna Letters is unanimous for **atta*. Rainey says, “The spellings are *at-ta* without exception”. Strictly speaking, there are a couple of exceptions, and Rainey (1996, p. 56) properly reports them, explaining them (away?) as scribal errors. That is only to be expected, and there would be no need to fuss about that were it not for the danger of taking the easy way out with other inconsistent evidence, and setting it aside as the exception that proves the rule.

So far as the terminal vowel is concerned — and this is what we are interested in here — **-a* dominates. It has always been a puzzle, at least for the Neogrammarians, to account for the existence in later Hebrew and Aramaic of both **att* and **attā*. The labelling of the vowel as anephs (Bauer and Leander 1922, p. 249) does little more than recognize that problem, or rather to push it back into prehistory. The anomaly is the survival of an originally short word-terminal vowel by lengthening,

⁴⁹ Levine 1993, pp. 38-44.

when such vowels were generally lost. Attempts to explain the divergent outcome in terms of different stress patterns are *ad hoc* and unconvincing, especially since the feminine form behaved differently from the masculine. The remedy lies in recognizing that “particles”, that is, members of parts of speech with small membership, are less vulnerable to the combined forces of phonologically driven sound changes that spread by paradigmatic analogy. In the case of the 2nd person free pronouns, the loss of final vowels would lose the distinction between masculine and feminine. The vowels could survive in that position only if they were long (this happened in Akkadian); but only one need be preserved to secure the gender distinction. But, in so far as the plural pronouns had to serve as common as well as masculine, the gender distinction could be lost. This happened in Hebrew in the 3rd person of perfect verbs, where the gender distinction held up in other languages. The gender distinction of the 2nd sg. pronoun forms, secured originally by the contrast in the final vowels, and preserved in Akkadian *atta* and *atti* and in Ethiopic *’atta* and *’atti* could still be preserved when final short vowels began to disappear if both *’atta* and *’atti* lengthened the final vowel, or if only one did (as happened in Hebrew *’attā* and *’att*). But even if both became *’att* (as eventually in Aramaic [Lipiński 1997, pp. 298-99]) the language could still manage with the ambiguity. There is no need to go into more detail. Each Semitic language has its own peculiarities, so far as the pronoun system is concerned. So it is entirely plausible that similar divergence would have taken place *within* the dialects of Hebrew.

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Tripartite Nominal Clauses and Appositional Clauses in Biblical Hebrew

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Abstract

This paper confronts one aspect related to nominal clauses in Biblical Hebrew, namely, the border line between the so called tripartite nominal clause that includes an independent personal pronoun, such as וַיֹּסֶף הוּא הַשְּׁלִיט עַל-הָאָרֶץ — “Now Joseph was the vizier of the land; it was he who dispensed rations to all the people of the land” (Gen. 42:6), and a nominal construction involving an appositional clause, such as וְשֵׁם-הַנָּהָר הַשֵּׁנִי גִיחוֹן הוּא — “The name of the second river is Gihon, the one that winds through the whole land of Cush” (Gen. 2:13). The purpose of the paper is to make a clear distinction between the two. The discussion also examines instances such as וְהַנָּהָר הָרְבִיעִי הוּא פָּרָת — “And the fourth river is the Euphrates”/ “And the fourth river, that is the Euphrates” (Gen. 2:14), and regards them not as nominal clauses in the order ‘subject — an independent personal pronoun — predicate’ but as similar to other constructions containing a head and an appositional clause in the order ‘an independent personal pronoun/predicate — subject’.

I.

The nominal clause structures and the use of the copula in Biblical Hebrew have been at the center of many studies. These questions were dealt with by Driver (1892),¹ Andersen (1970), Muraoka (1985,² 1990, 1998), Nic-

¹ Driver 1892, pp. 264-274, §196-§201.

² Muraoka 1985, pp. 67-77.

cacci (1993, 1999), and Zewi (1994, 1996), among many others. They were also part of comprehensive syntactical descriptions of Biblical Hebrew grammar, found in Kautzsch,³ Joüon,⁴ Waltke & O'Connor,⁵ and others. A new book edited by Miller⁶ contains many papers on this topic, which present reviews of previous examinations as well as new insights.⁷

This paper confronts only one aspect of these questions, namely the borderline between the tripartite nominal clauses⁸ that include independent personal pronouns on the one hand, and nominal constructions involving appositional clauses by means of independent personal pronouns on the other. Through an analysis of these two constructions the paper presents two types of nominal clauses, in which the predicate is in initial or in second position, and it confronts the question of whether there exists in Biblical Hebrew a tripartite nominal clause in which the predicate is in a third position.

2. Bipartite Nominal Clauses and Apposition

The difficulty in recognizing which structure presents a nominal clause and which an apposition is itself an old problem in Semitic languages. It arises not only regarding structures that contain independent personal pronouns, but regarding bipartite nominal clauses versus appositions lacking any independent personal pronouns as well. Thus, in nominal clauses like **הָעֵם אֲכֵן הָצִיר** — “Indeed, man is but grass” (Isa. 40:7),⁹ or **טוֹב הַדְּבָר** — “It is fair” (1 Kgs. 2:42), one can easily tell the difference between subject and predicate by considering the definite member as subject and the indefinite as predicate. Moreover, the existence of one definite member and one indefinite allows immediate recognition of a predicative relation between the two.

By contrast, when a text exhibits two members, both indefinite or both definite, appears a real difficulty in recognizing whether the relation between the two is predicative or attributive, that is, whether the two mem-

³ Kautzsch 1910:451-455, §141.

⁴ Joüon 1947, pp. 466-472, §154, and Joüon & Muraoka 1996, pp. 564-577, §154.

⁵ Waltke & O'Connor 1990, pp. 130-135, §8.4, pp. 297-299, §16.3.3.

⁶ Miller 1999a.

⁷ See Miller's remarks on this ongoing discussion in Miller 1999b, pp. 13-14, §3.3.

⁸ In this paper I use the term ‘tripartite nominal clause’ only as a label for nominal clauses containing an independent personal pronoun. In my opinion, there are no real tripartite nominal clauses in Biblical Hebrew. The tripartite nominal clauses actually are extrapositions reflecting two levels of bipartite structure, *i.e.* ‘predicate clause, subject’ or ‘subject, predicate clause’ and a predicate clause constructed of ‘predicate-subject’.

⁹ Translations of Biblical verses are mainly according to the *JPS*.

bers form a complete sentence or only a phrase. In such cases an indication of a predicative relation or an attributive relation relies on context and context only.¹⁰ For instance, there is no way to tell whether **אֲנֹכִי רוּת אֲמָתְךָ** from Ruth 3:9 is an apposition in a longer sentence or a nominal clause, except through analysis of the context. The full context is **וַיֹּאמֶר מִי־אַתָּה וַתֹּאמֶר אֲנֹכִי רוּת אֲמָתְךָ** — “Who are you? He asked. And she replied, I am your handmaid Ruth” (Ruth 3:9). The fact that the words **אֲנֹכִי רוּת אֲמָתְךָ** are definite does not allow any decision, but since these words are a reply to a question, and since there is no other sentence part capable of fulfilling the predicate role, one can draw the conclusion that **אֲנֹכִי רוּת אֲמָתְךָ** is a full nominal clause rather than a sheer phrase.¹¹

3. Tripartite Nominal Clauses and Appositional Clauses

The problem regarding bipartite nominal clauses and appositions is a little different from that regarding the tripartite nominal clauses and appositional clauses involving independent personal pronouns. In such cases, too, the distinction between a full sentence and an apposition has to rely on context, but the constructions are longer and more complicated. Since the nominal clause types relevant for this discussion are those exhibiting the independent personal pronoun in the middle, between the two other sentence main parts, only these types are presented below.

One type of nominal clause containing an independent personal pronoun between the subject and the predicate in Biblical Hebrew is basically constructed of three main parts in the order ‘predicate — an independent personal pronoun — subject’. In this type the predicate is mostly indefinite, the subject is mostly definite, and instead of the independent personal pronoun a demonstrative pronoun might occur. Instances are, *e.g.* **אֵלֶּה הֵם הָאֱלֹהִים הַמֵּכִים אֶת־מִצְרַיִם בְּכָל־מָכָה בַּמִּדְבָּר** — “He is the same God who

¹⁰ Niccacci (1999, p. 245) says the following: “Both *casus pendens* and *apposition* are traditional terms. In modern terminology they are called *topicalization* or *left- (or front-) dislocation* and *right- (or rear-) dislocation*, respectively.” These words indicate that Niccacci, following others, does not discern between *casus pendens* which involves a predicative relation and *apposition* which involves an attributive relation, though he himself states that he prefers the traditional terms (*ibid.*, pp. 245-246). However, formerly, Niccacci rightly cites a definition which considers *apposition* “at the margin of the dependency, subordination relation” (*ibid.*, p. 228).

¹¹ The problem of differentiating between a nominal clause and an apposition in similar cases arises in ancient Semitic inscriptions that open with a presentation of a king. Such is the case in, *e.g.*, Meša’ Inscription, line 1 — *’nk.mš.bn.kmš[yr] mlk.m’b.hdybny*, which may be translated either as “I am Meša’, the son of Chemosh, king of Moab, the Dibonite” or “It was I, Meša son of Chemosh, king of Moab, the Dibonite, who...” (Andersen 1966, pp. 112-113).

struck the Egyptians with every kind of plague in the wilderness” (1 Sam. 4:8), *זֶה הוּא בֵּית ה' הָאֵלֹהִים* — “Here will be the house of the Lord” (1 Chron. 22:1). The *JPS* English translations presented above do not reflect very well the true structure of these instances. In fact, the first word of these verses should be read with stress, which marks the actual predicate. Such a sentence is typical of questions, *e.g.*, *מִי־אִפּוֹא הוּא הַצֶּדֶד־צִיד* — “Who was it then that hunted game?” (Gen. 27:33), *בֶּן־מִי־זֶה הַנֶּעֶר אֲבִנֶר* — “Whose son is that boy, Abner?” (1 Sam. 17:55). This type of nominal clause is actually constructed of a predicate clause and a following extraposed subject. The predicate clause is constructed of a predicate and an independent personal pronoun or a demonstrative pronoun as its subject.¹²

Another type of nominal clause involving an independent personal pronoun between the two other main parts of the sentence is more complicated. Superficially, it might be presented as having the sequence ‘definite nominal phrase — an independent personal pronoun — definite nominal phrase’. Since the two main parts of the sentence are definite it might be argued that there is no way to tell which is the subject and which is the predicate, and consequently both interpretations are possible. However, this sentence type presents a clear direction of predicative relation, and its subject and predicate can be clearly discerned. Its true structure should be interpreted as constructed of ‘an extraposed subject — a predicate clause’. The predicate clause is built in the sequence ‘predicate — subject’, and the predicate of the predicate clause is the independent personal pronoun, which also functions as a retrospective pronoun referring to the extraposed subject.¹³ Instances are, *e.g.*, *יֹוסֵף הוּא הַשְּׁלִיט עַל־הָאָרֶץ הוּא הַמְּשָׁבִיר לְכָל־עַם*, *הָאָרֶץ* — “Now Joseph was the vizier of the land; it was he who dispensed rations to all the people of the land” (Gen. 42:6), *כִּי ה' אֱלֹהֵיכֶם הוּא הַנִּלָּחֵם*, *לָכֶם* — “For it is the Lord your God who will battle for you” (Deut. 3:22, and similarly in Josh. 23:3, 10). The stress in such nominal construction probably was on the independent personal pronoun, that is, the second member, contrary to the previous nominal constructions, in which the prominent intonation probably was on the first member.¹⁴

¹² See discussion of this type in Zewi 1994, pp. 160-162, §10 (entitled type C). Compare with Muraoka 1985, p. 76 (the type entitled P-C-S) and Joüon & Muraoka 1996, pp. 574-575, §154j (the type entitled P-Pron.-S).

¹³ Zewi 1994, pp. 162-164, §11 (entitled type D).

¹⁴ The ground for presenting two types of nominal clauses with an independent personal pronoun in the middle, contrary to Muraoka who presents only one (Muraoka 1995, pp. 72-75, 1998, p. 201), is (1) that wherever it is possible to discern between definiteness and indefiniteness, the first nominal phrase of nominal clause type 1 is mostly indefinite, while the first nominal phrase of type 2 is mostly definite, and (2) the supposed position of the stress in the

These two types, which contain an independent personal pronoun between the two main parts of the sentence, might be sketched as follows:

type 1: predicate clause — subject

/ \
predicate — subject
 \
 (personal pronoun)

type 2: subject — predicate clause

/ \
(predicate — subject)
/
(personal pronoun)¹⁵

These two nominal clause types are well represented in Biblical Hebrew. In both of them the independent personal pronoun is between two nominal phrases. Nevertheless, the direction of these types in terms of predicative relation is clear, and in both of them the last member of the sentence is a subject.

4. Appositional Clause, Exegetical Remark, Gloss

Now, in Biblical Hebrew there also appears another sequence, ‘nominal phrase — an independent personal pronoun — nominal phrase’, which takes part in a larger sequence. Instances of such constructions are as follows: וְשֵׁם הַנָּהָר הַשְּׁלִישִׁי חֲדָקִל הוּא הַהַלֵּף קְדֵמַת אֲשׁוּר — “The name of the third river is Tigris, the one that flows east of Asshur” (Gen. 2:14), וְתֵלֵד הַבְּכִירָה בֶּן וְתִקְרָא שְׁמוֹ מוֹאָב הוּא אָבִי-מוֹאָב עַד-הַיּוֹם — “The older one bore a son and named him Moab; he is the father of the Moabites of today” (Gen. 19:37), וְתָמָת שָׂרָה בְּקִרְיַת אַרְבַּע הוּא חֶבְרוֹן בְּאֶרֶץ כְּנָעַן — “Sarah died in Kiriath-arba — now Hebron — in the land of Canaan” (Gen. 23:2), וַיֵּצֵב יַעֲקֹב מִצְבֵּה עַל-קִבְרֹתָהּ הוּא מִצְבֵּת קִבְרַת-רָחֵל עַד-הַיּוֹם — “Over her grave Jacob set up a pillar; it is the pillar at Rachel’s grave to this day” (Gen. 35:20), וַיֵּסְעוּ מֵעִזְיוֹן גִּבֵּר וַיַּחֲנוּ בְּמִדְבַּר-צִן הוּא קָדֵשׁ — “They set out from Ezion-geber and encamped in the wilderness of Zin, that is, Kadesh” (Num. 33:36) מֵעֶרְוֶר אֲשֶׁר עַל-שְׂפַת-נַחַל אֲרֹנוֹן וְעַד-הָר שִׂיֵּאֵן הוּא חֶרְמוֹן — “From Aroer on the banks of the wadi Arnon, as far as Mount Sion, that is

clause varies according to type. It is on the first member (=nominal phrase) in type 1 and on the second member (=independent personal pronoun) in type 2.

¹⁵ Zewi 1994, p. 164, §12.

וְעֵלָה הַנִּבּוּל גַּי בֶּן־הַנֶּם אֶל־כֶּתֶף הַיְבוּסִי מִגִּבּוֹר הָיָא (Deut. 4:48), — “Then the boundary ascended into the Valley of Ben-hinnom, along the southern flank of the Jebusites — that is Jerusalem” (Josh. 15:8).
וַיִּבֶן עִיר וַיִּקְרָא שְׁמָהּ לוֹ הוּא שְׁמָהּ עַד הַיּוֹם הַזֶּה — “He founded a city and named it Luz, and that has been its name to this day” (Judg. 1:26), וַיִּלְכֹּד — “But David captured the stronghold of Zion; it is now the City of David” (2 Sam. 5:7).
בַּחֹדֶשׁ זוֹ הוּא הַחֹדֶשׁ הַשְּׁנִי — “In the month of Ziv — that is, the second month — in the fourth year of his reign over Israel” (1 Kgs. 6:1), וַתִּקְרָאנָה — “They named him Obed; he was the father of Jesse, father of David” (Ruth 4:17), וַיְהִי בִימֵי אַחַשְׁוֵרוּשׁ הוּא — “It happened in the days of Ahasuerus — that Ahasuerus who reigned over a hundred and twenty-seven provinces from India to Nubia” (Esther 1:1), וַאֲנִי הָיִיתִי עַל יַד הַנָּהָר הַגָּדוֹל הַזֶּה — “When I was on the bank of the great river — the Tigris” (Dan. 10:4).¹⁶

In these instances the syntactical relation between the independent personal pronoun and the preceding nominal phrase is not predicative but attributive. The appositional clause itself is a full clause constructed of an independent personal pronoun as predicate and another nominal phrase as its subject.¹⁷ The most prominent feature is that in the majority of the instances the appositional clause adds information regarding the identification or identity of a proper noun, mostly a name of a person or a name of a place.¹⁸ It should also be noted that this construction is found only in biblical narrative prose; it is very common in Genesis, Joshua, and especially

¹⁶ More instances are Gen. 2:11,13, 14:2,3,7,8,17, 19:38, 23:19, 35:6,19,27, 36:24, 48:7, Num. 26:9, Josh. 15: 8,9,12,24,48,53,59, 18:12,13,27, 20:6, 21:10, Judg. 7:1, 19:10, 2 Sam. 23:8, 1 Kgs. 6:17,38, 8:1,2, 2 Kgs. 18:9,10, 25:8, Jer. 25:1, 32:1, 52:12, Ezek. 2:1, Zech. 1:7, Esther 2:7, 16, 3:7,13, 8:9,12, 9:1, Ezra 1:3, 7:9, 10:9, 1 Chron. 1:27, 2:26,42, 4:11, 5:6, 5:8,36, 7:31, 8:7,12,13, 9:31, 11:4,5,11,12, 2 Chron. 5:2,3, 18:7, 20:2.

¹⁷ The personal pronoun is regarded as the predicate and the additional information as the subject, although it is new in the sentence, since the fact that this new information is related to the previous nominal phrase, explained by the aid of the pronoun, is newer. Therefore, most nominal phrases following the personal pronoun are definite, as, e.g., הַהֶלֶךְ קְדָמָתִי, אֲשׁוּר — “...the one that flows east of Asshur” (Gen. 2:14).

¹⁸ One instance which might be constructed in a similar way, although it is not related to a proper name, is לֵיל שְׁמֵרִים הוּא לַ' הַלְוִיָּאִים מֵאַרְצָּה מִצְרַיִם הוּא־הַלֵּילָה הַזֶּה לַ' שְׁמֵרִים לְכָל־ — “That was for the Lord a night of vigil to bring them out of the land of Egypt; that same night is the Lord’s, one of vigil for all the children of Israel throughout the ages” (Exod. 12:42). For this instance also see Kogut 1982, p. 104, §2.3.1. A similar appositional clause not related to proper names might also exist in וְתָהֵם הַקְרִיָּה הוּא הַקוֹל אֲשֶׁר — “And the city went into an uproar. That’s the noise you heard” (1 Kgs. 1:45), and in שְׁמַעְתֶּם — “And the city went into an uproar. That’s the noise you heard” (1 Kgs. 1:45), and in הָפִיל פֹּרֶה הַנוֹרֵל לְפָנֵי הַכֶּן — “pur” which means “the lot” — was cast before Haman” (Esther 3:7, and similarly 9:24).

Chronicles, in which many proper names are mentioned, and exists only sporadically in other books. A smaller group of instances employs this type of appositional clause for adding more information to a specific identification of a certain month or year.¹⁹ This type is found only in 1 and 2 Kings and in Later Prophets and other later biblical books. These constructions are appositional clauses from a syntactical point of view, and their function is to add information or explanation to a proper noun. Thus, from a textual point of view they are frequently called exegetical remarks or glosses.²⁰

Confusion of constructions containing heads followed by appositional clauses with the two nominal clause types, constructed of ‘a nominal phrase — an independent personal pronoun — a nominal phrase’, is possible, since the independent personal pronouns in these appositional clauses should probably also be considered as stressed. However, whenever appositional clauses are involved, the first nominal phrase, *i.e.* the head, always has other syntactical relations with additional sentence members.

5. וְהַנָּהָר הָרְבִיעִי הוּא פָּרַת — A Tripartite Nominal Clause or a Head Followed by an Appositional Clause?

Now, the question is whether a third nominal clause type exists in which the word order is ‘subject — an independent personal pronoun — predicate’.²¹ Instances that have been brought up in relation to this question usually are the following four.

The most important case for asserting that a nominal clause exists in the sequence ‘subject — an independent personal pronoun — predicate’ in Biblical Hebrew seems to be the clause וְהַנָּהָר הָרְבִיעִי הוּא פָּרַת — “And the fourth river is the Euphrates” (Gen. 2:14).²² This instance is part of a larger

¹⁹ These are 1 Kgs. 6:1,17,38, 8:2, 2 Kgs. 18:9,10, 25:8, Jer. 25:1, 32:1, 52:12, Ezek. 2:1, Zech. 1:7, Esther 2:16, 3:7,13, 8:9,12, 9:1, Ezra 7:9, 10:9, 2 Chron. 5:3, which are cited among the other instances above.

²⁰ For the terms ‘exegetical remark’ and ‘gloss’ for this construction see Muraoka 1990, p. 229, §1.2.1, Tov 1994, pp. 52-53 and Baasten 1997, p. 4, and *ibid.* note 12. In Muraoka 1998, p. 187 the term ‘parenthetical gloss’ is employed. Syntactically, this construction can be regarded as an asyndetic relative clause as well. In Muraoka 1990, p. 229, §1.2.1, for instance, this structure is compared to a syndetic relative clause in Mishnaic Hebrew.

²¹ A sequence S-C-P is suggested in Muraoka 1985, pp. 72-77, but later he avoids the term ‘copula’ since he believes that there is no real copula in Biblical Hebrew or Classical Semitic languages, and in Joüon & Muraoka 1996, pp. 574-575, §154j the sequence P-Pron.-S is suggested. I would like to thank Prof. Muraoka for his comment regarding this topic in private communication.

²² Thus suggests Muraoka for this instance in Muraoka 1985, p. 72, 1998, p. 198. See also Bendavid 1971, p. 729.

verse exhibiting an appositional clause in its other part **וְשֵׁם הַנָּהָר הַשְּׁלִישִׁי** — **חֲדָקְלָהּ הוּא הַהַלֵּךְ קִדְמַת אַשּׁוּר** — “The name of the third river is Tigris, the one that flows east of Asshur”, and it is bordered by more similar constructions exhibiting appositional clauses in Gen. 2:11, 13. This instance also refers to a proper noun exhibiting a name of place. Geller rightly indicates that the River Euphrates was more famous than the other rivers mentioned close by.²³ Consequently, the structure of this clause as suggested by Geller might be an extraposition. However, based on this very conclusion of the Euphrates being famous or better known, the predicate clause of this extraposition, **הוּא פָּרַת**, should be translated “that is the Euphrates” with a stress on the personal pronoun. The predicate of this predicate clause is in the order predicate- subject, in which the independent personal pronoun is the predicate and the proper noun ‘Euphrates’ is the subject.²⁴ Another possibility is to regard the verse part **וְהַנָּהָר הָרְבִיעִי הוּא פָּרַת** not as a complete sentence but only as a phrase exhibiting a gloss, which is added to the first part of the sentence. In this case it should be translated “...and the fourth river, which is the Euphrates”. This last suggestion conforms well with all other glosses discussed above, most of which are related to proper names.

A second instance that can be interpreted as a nominal clause in the sequence ‘subject — an independent personal pronoun — predicate’ is found in the following verse, **וַיֵּהְיוּ בְנֵי-נֹחַ הַיִּצְאָאִים מִן-הַתֵּבָה שֵׁם נֹחַ וַיֵּפֶת וְחָם** — **הוּא אָבִי כְנָעַן** — “The sons of Noah who came out of the ark were Shem, Ham, and Japheth — Ham being the father of Canaan” (Gen. 9:18). The second part of this verse: **וְחָם הוּא אָבִי כְנָעַן**, can be translated as a nominal clause — “And Ham is the father of Canaan”.²⁵ However, this instance is related in context to proper name attributed by appositional clauses. The consideration of this construction as a complete nominal clause is due to the repetition of the name ‘Ham’, but this repetition might be only for adding more information to this name. The phrase **וְחָם הוּא אָבִי כְנָעַן** might also be translated as “And Ham, who is the father of Canaan” or “Ham being the father of Canaan”, as cited above according to the *JPS* translation. As to the new information in this phrase, one should ask whether it is ‘father of Canaan’ or the fact that this information is related to Ham. If the last part is more important, the stress should be again on the independent personal pronoun and not on the last nominal phrase.

²³ Geller 1991, p. 32.

²⁴ This structure is similar to tripartite nominal sentence of type 2 mentioned above in §3.

²⁵ This instance is cited by Muraoka in Muraoka 1985, p. 72 under the order S-C-P, and see also Muraoka 1998, p. 198.

A third instance that could be interpreted as a nominal clause appears in the following: וַיֹּאמֶר אַבְרָם אֲדֹנָי ה' מֶה־תִּתֶּן־לִי וְאֲנֹכִי הוֹלֵךְ עֲרִירִי וּבֶן־מֶשֶׁק — “But Abram said, O Lord God, what can you give me, seeing that I shall die childless, and the one in charge of my household is Dammesek Eliezer” (Gen. 15:2). The verse part וַיֹּאמֶר אַבְרָם מֶשֶׁק בֵּיתִי הוּא is translated by the *JPS* above as a nominal clause.²⁶ In this verse this nominal clause appears out of context, since it is expected that Abram will say that he has a man in charge of his household who is not a descendant, and the addition of his personal name as predicate is irrelevant. However, if one considers the proper name as the known information, and the new information in the connection of this information to the man in charge of the household, it is better understood. In this case the independent personal pronoun is again the sentence member that carries the stress. Anyway, this instance is also related to a proper name, and it recalls all other instances of appositional clauses attached to proper names.

The last instance is וַיֵּשֶׁב עֵשָׂו בְּהָר שִׁעִיר עֹשֶׂה הוּא אֶדּוֹם — “So Esau settled in the hill country of Seir — Esau being Edom” (Gen. 36:8). The sentence part עֹשֶׂה הוּא אֶדּוֹם could be translated as a nominal clause, as follows: “Esau is Edom”.²⁷ However, once more, this verse is related to a proper name. The second name ‘Esau’ repeats its first occurrence in a construction which is, in fact, an apposition. Like the construction that includes the repetition of the name ‘Ham’ in Gen. 9:18, this apposition contains another apposition to the proper name, one that opens with the independent personal pronoun and forms an appositional phrase. Moreover, in this case, other three instances appear in the same chapter, in all of which a similar construction cannot be interpreted except as an appositional clause. These instances are as follows: וְאֵלֶּה תְּלִדֹת עֵשָׂו הוּא אֶדּוֹם — “This is the line of Esau — that is, Edom” (Gen. 36:1), אֵלֶּה בְּנֵי־עֵשָׂו וְאֵלֶּה אֱלֹפִיָּהֶם הוּא אֶדּוֹם — “Those were the sons of Esau — that is, Edom — and those are their clans” (Gen. 36:19), אֵלֶּה אֱלֹפִי אֶדּוֹם לְמִשְׁבְּתָם בְּאֶרֶץ אַחְזָתָם הוּא עֵשָׂו אָבִי אֶדּוֹם — “Those are the clans of Edom — that is, of Esau, father of the Edomites — by their settlements in the land which they hold” (Gen. 36:43).

On first looking at all these instances one is tempted to consider them as true nominal clauses in the order ‘subject — an independent personal pronoun — predicate’. However, it is more than likely that such a construction

²⁶ This instance is cited by Muraoka in Muraoka 1985, p. 72 under the order S-C-P as well. See also Bendavid 1971, p. 729.

²⁷ For a translation “Esau(, he) is Edom” with an extraposed subject, see Waltke & O’Connor 1990, p. 131, §8.4.1, p. 297, §16.3.3. This translation also considers this construction as exhibiting a preciative relation.

does not exist in Biblical Hebrew. All the four instances that are occasionally considered as representing this sequence show similarity in structure and context to the very common type of appositional clauses/glosses that is mostly attached to proper nouns. As for structure, in all four instances (Gen. 2:14, 9:18, 15:2, 36:8) the independent personal pronoun should be regarded as carrying the intonation, so that in structure they resemble nominal clause type 2. Three instances might be interpreted as appositional clauses. These appositional clauses are constructed of short bipartite nominal clauses following a noun serving as their head. They are not tripartite nominal clauses (Gen. 2:14, 9:18, 36:8). Moreover, the first one follows appositional clauses (Gen. 2:14), and the fourth is parallel to other appositional clauses revealing similar contents (Gen. 36:8). As for context, all four instances are related to proper names, which are the most frequent context of such appositional phrases.²⁸

6. Final Words

It is possible to describe three types of constructions involving an independent personal pronoun in Biblical Hebrew, two being tripartite nominal clauses and one containing an appositional clause. In the first two nominal clause types involving an independent personal pronoun between two nominal phrases the third sentence member, namely the second nominal phrase, is a subject. A tripartite nominal clause in which the third member, namely the second nominal phrase, is a predicate does not exist in Biblical Hebrew. Four instances which might present such a sequence are actually constructed according to nominal clause type 2 in which the third member is a subject. Three of these instances might also be interpreted as containing appositional clauses, and all four are clearly related to other appositional clauses in Biblical Hebrew as regards context and partial structure.

²⁸ One more instance that might be related to the four discussed above is וְדָוִד הוּא הַקָּטָן (Muraoka 1985, p. 73, 1998, p. 198), which is part of the following, וְשֵׁם שְׁלֹשֶׁת בָּנָיו אֲשֶׁר הָלְכוּ — “The names of his three sons who had gone to the war were Eliab the first-born, the next Abinadab, and the third Shammah; and David, he was the youngest” (1 Sam. 17:13-14, the translation of the last part as an extraposition is mine). Following my suggestions regarding Gen.2:14, I see this sentence as a tripartite sentence of type 2 with the stress on the independent personal pronoun (§3 above). Moreover, I think that this instance can also be interpreted as including an appositional clause/gloss, and thus be translated not as a full clause but as a phrase in the following manner: “...and David, who is the youngest.” Such a translation goes well with the context, mentioning David as another son who went to the war. The context is related here again to proper names.

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Edomite Westward Expansion: The Biblical Evidence*

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Abstract

This paper aims to complement the archaeological research on Edomite settlement west of the Arabah, by attempting to demonstrate from the biblical texts that Edomite pressure on the Judahite south was a long term phenomenon. The data are drawn from the narratives concerning five Judaeans kings who reigned during the ninth to the sixth centuries BCE, as well as from the book of First Esdras. Special attention is given in an excursus to texts where the rendering of the names 'Edom' and 'Aram' are open to question. I conclude that the Edomites, after several setbacks, but with Assyrian and Babylonian help, eventually succeeded in establishing themselves in the Judahite south.

Research on Edom in the past few years has highlighted discoveries at sites in the Negev which have yielded material such as a shrine, cultic vessels, pottery, ostraca and figurines. Debate has centred on the meaning of these finds. Do they indicate the presence of Edomites as invaders, traders, migrants or refugees in the lands to the west of their homeland?

The treatment of Edom's westward expansion has been dealt with in a piecemeal fashion, until recently. Glueck early on dismissed textual evidence as late glosses when discussing Edom's boundaries.¹ But the recent archaeological work both in the Arabah and the Judahite Negev shows that possible Edomite settlement or occupation, certainly influence, took place

* The following abbreviations are used: BHS, Biblia Hebraica stuttgartensia. DCH, Dictionary of Classical Hebrew. NRSV, New Revised Standard Version, RSV, Revised Standard Version. NEB, New English Bible. REB, Revised English Bible.

¹ Glueck 1936, pp. 141-157.

at 'En Haseva, Horvat Qitmit, Tel Masos, Tel 'Aroer, Kh. 'Uza, Tel Malhata, Tel 'Ira, Tel Arad, Horvat Radum, Tel Sera', Tel Haror, Kadesh Barnea and Tell Jemmeh (Fig. 1). The evidence from some of these sites has been very cursorily reported and dealt with, but important articles have been written dealing with major finds.²

To complement the evidence of the archaeological work, I feel a fresh look at the textual evidence is called for. The Hebrew texts, critically examined, are still one of our main sources of information concerning westward expansion of Edomites into the Judahite Negev. The purpose of this paper is to deal with this topic in the light of recent incremental research and the biblical texts. I believe that it can be shown that the process of expansion was made over a considerable period of time, beginning approximately in the mid-ninth century BCE. Thus, it can be argued that Edomite pressure on the Judahite south was a long-term trend. The pressure was applied at various and strategic periods over several centuries, chiefly because the Edomites discerned the possibilities of exploiting trade opportunities with Arabia, whose incense trade routes ran through Edomite territory. It eventually met with a good measure of commercial success after the Babylonian exile, when deportations facilitated Edomite settlement west of the Arabah. There were also opportunities for the commercial exploitation of Edom's copper resources which were to be found around Feinan. Accordingly, we shall examine the textual evidence and bring in other relevant data where appropriate.

II Chronicles 20:1-2. The Reign of Jehoshaphat 870/869-848 BCE.

Probably the earliest attempt at westward expansion is that mentioned in II Chronicles 20:1-2, (*Cf.* also II Chron. 10.22-23) in the reign of Jehoshaphat (870-848 BCE) where, during the course of the Moabite war (probably ca. 848 BCE, Jehoshaphat's last year, see below note 10), the coalition of Moab, Ammon and some Meunites invaded the Judahite wilderness area, around the oasis of En-Gedi. The passage reads:

After this the Moabites and Ammonites, and with them some of the Meunites, came against Jehoshaphat for battle. Messengers came and told Jehoshaphat, 'A great multitude is coming against you from Edom, from beyond the sea; already they are at Hazazon-tamar' (that is, En-Gedi).

The Meunites mentioned in the passage are identified in vv. 10 and 22 as "men of Mt Seir", a term that in a number of places stands as a parallel

² Mazar 1985; Beit Arie 1988, 1995, 1996; Finkelstein 1992; Cohen & Yisrael 1995.

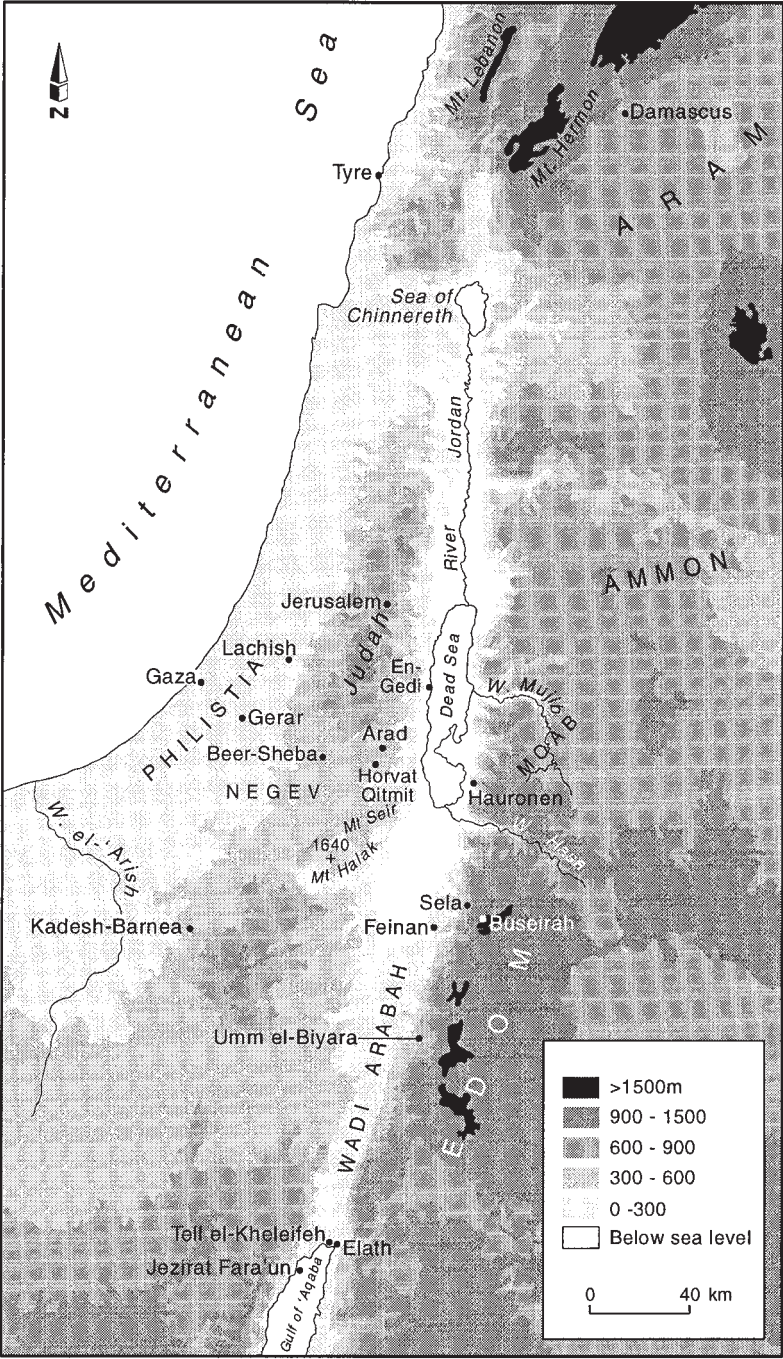


Fig. 1. Edom and its neighbours: Some key sites.

term to Edom as a geographical area (e.g. Gen. 32:3, 36:8; Numb. 24:18; Judges 5:4; Ezek. 35:15). This accords with the description in verse 2 where Jehoshaphat is told, "A great multitude is coming against you from Edom..." The description of the location of the attack on the territory of Judah's south is accurate and in the words of one commentator, the details "provide a solid geographical background for the narrative".³ The attack contained a good element of surprise for the Transjordan confederates came "from beyond the sea" (me'eber layyam); that is, they attacked En-Gedi on the western shore of the Dead Sea by crossing the ford that existed in those days in periods of low rainfall between the el-Lisan peninsula and the west shore.⁴

But as regards the identity of some of the attackers, the question needs to be asked: 'are the Meunites Edomites?' The passage is problematic for two reasons. One, the reading 'Edom' in verse 2 which relies on the evidence of one MS where the MT instead reads 'Aram'; and two, on the reader accepting the Chronicler's ostensibly intended identification of 'Meunites' with 'men of Mt Seir'. What do we know about the Meunites? Here there is some controversy. According to one commentator, the Chronicler possibly had in mind the people of Maon, the present day Ma'an, a town about 25 km south-east of Petra, in the Edomite *midbar* (arid wilderness area).⁵ But this suggestion does not tie in with the description of the Meunites in the Chronicler (II Chron. 26:7 and I Chron. 4:41) which locates them west, and not east, of the Arabah. Nor does it square with the identification of the Meunites in the Assyrian texts where they are placed south and south-east of Philistia.⁶ So, then, we are obliged to surmise that the Meunites/Mt Seirites were probably dwelling as far east as Mt Halak in the southern Negev. As Williamson puts it:

"[The Meunites'] link with Mount Seir is no objection... for there have always been those who have argued that Seir was located somewhere in the southern Negeb, and in recent years strong arguments have been advanced in its support; cf. J.R. Bartlett, *JTS* ns 20 (1969), pp. 1-20, and G.L. Davies, *PEQ* III (1979), pp. 97-100."⁷

It would appear, then, that the Meunite group joined up with their Moabite and Ammonite allies after a northward march through the Edomite hill country in order to approach En-Gedi across the el-Lisan ford.

³ Japhet 1993, p. 787.

⁴ See Mazar, *et al.* 1966, p. 2, note 6. Cf. Aharoni & Avi-Yohah 1968, p. 58 and map 133.

⁵ See Williamson 1982, p. 294.

⁶ See for a detailed discussion Knauf 1992, pp. 891f. Eph'al 1984, pp. 219f. Na'aman 1979, p. 70. Bartlett 1989, pp. 143-145.

⁷ Williamson 1982, p. 294. I believe Diana Edelman also has made a good case for Seir being in this area. See Edelman 1995, p. 5, n. 20.

But again how does this scenario support the contention that the Meunites were Edomites? I tend to agree with Japhet's statement that while the Meunites are connected with Mount Seir, the rest of the Chronicler's references together with the evidence of the Assyrian inscriptions form

... a quite impressive uniformity of references, while their precise location — on the east or the west side of the Arabah — cannot be conclusively determined, and may have actually shifted through the years. Here they are connected with 'Mount Seir', but it is clear throughout that they are distinct and separate from the Edomites.⁸

So, in what sense can we say that this incident can be regarded as the beginning of Edomite westward expansion? Probably in this way: the Meunites are linked with Moab and Ammon which *are* Transjordanian states. And recall that access to the el-Lisan is reached from Moab, yet v.2 says "from Edom, from beyond the sea"! Now that means either that the attack was initiated by Edom[ites]; or it means that that was the direction from which the armies came. Probably the latter is meant. And an approach *through* and/or '*from* Edom' can mean that Edom had some hand in the attack (compare by way of illustration the incident in Numbers 20:21 of Edom's refusal to *allow passage* through their land). So, we might argue, there is some justification for seeing this incident as being the beginning of the western encroachments by Edom.⁹

There is another aspect of the biblical evidence to consider. The II Chronicles 20 passage indicates an important change in the political relations between Judah and Edom. At this very period of time in Edom, if we are to accept the evidence of other biblical texts, an appointee of Jehoshaphat was supposed to be exercising control for the benefit of Jehoshaphat and Judah. I Kings 22:47 states that during the course of Jehoshaphat's reign: "There was no king in Edom; a deputy was king" (NRSV — "only a viceroy of Jehoshaphat" [REB]). The Chronicles' picture, suggested by referring to this text in Kings, is that Jehoshaphat's appointee was losing his grip, and his control over Edom was weakening, so that the attack by the Transjordanian allies was able to strike at Judah "from Edom". The corollary to this, of course, is the statements of II Kings 8:20 and II Chronicles 21:8 that, later, in the reign of Joram of Judah (Jehoshaphat's son) "Edom revolted against Judah and set up its own king".

The stated aim of the confederates' campaign appears to have been that of territorial acquisition, to judge from the words of Jehoshaphat in 20:11

⁸ Japhet 1993, p. 786.

⁹ For an ingenious ethno-theological interpretation of II Chronicles in biblical criticism's own terms, see Davies 1992, pp. 43-54.

("... they reward us by coming to drive us out of your possession that you have given us to inherit"). The action may be construed as a retaliation by Moab, in particular, for Judah's support of Jehoram of Israel's campaign against Mesha of Moab (II Kings 3) when Jehoram allied himself with Jehoshaphat and Jehoshaphat's deputy (viceroy) who controlled Edom (i.e. in II Kings 3:9 "the [appointed] king of Edom").¹⁰

The fact of the presence of Judah's forces in Moab and the assistance they gave may have found support in a new proposed reading in Mesha's stele, the Moabite Stone. André Lemaire's restoration of line 31 reads: "And *the house [of Da]vid* dwelt in Horonên (Hورانائم) [.....] and Kemosh said to me: 'Go down! Fight against Horonên'" makes it possible that we have here a reference to Judah's occupation of part of the area of Moab south of the Arnon (Wadi Mujib). The location of Horonên is not precisely fixed but it is in the territory of the el-Lisan peninsula hinterland. This new reading suggests that the latter part of the inscription, now largely missing, described Mesha's victory over the Judahite forces occupying sites southeast of the Dead Sea, just as the earlier parts of the stele mentioned Mesha's victory over the Israelite armies whom he defeated north of the Arnon.¹¹

It would appear at first sight from the story in II Chronicles 20 that no permanent gains came of this attempt on Judahite territory. There is no statement similar to that in II Kings 16:6, "...the Edomites came to 'X', where they live to this day"?¹² The allies, it would seem, fell out among themselves and indulged in an orgy of self destruction (II Chronicles 20:23). But the incident does illustrate the general intention of Edom to move west. In any case, it is possible that the Meunites and the Mt Seir group *did* manage to found settlements in the Judahite Negev, and even assimilate with some fringe dwellers there. This may be gauged from the description, about a century later, that there were what we might term 'Edomitic settlers' there who became victims of Hezekiah's push to reinforce the Negev against the projected Assyrian invasion under Sennacherib of 701 BCE. From I Chronicles 4:39-43 we learn that Hezekiah's push consisted of a group of Simeonites who struck westward toward Philistia and settled the area around Gerar where Meunites were settled; and another group which went eastwards and displaced the settlers of Mt Seir¹³ (see below, p. 7).

¹⁰ See Liver 1967, p. 27. He states that the probable date was 849 BCE which means that, if this retaliation suggestion is correct, the confederates' campaign would probably have been in Jehoshaphat's last year, i.e. 848 BCE. Cf. Thiele 1963, pp. 67 and 70.

¹¹ See Lemaire 1994, pp. 30-37. Cf. also Dearman 1989, p. 193, and the criticism of Na'aman 1997, pp. 89f.

¹² On the significance of the phrase 'until this day' see Tadmor and Cogan 1979, p. 497.

¹³ See Aharoni 1966 pp. 136f. Also Oded 1977, p. 445.

Now it is interesting and significant that this period which marks the start of the attempt at westward encroachment by Edom is also, according to Bartlett, the very period of the establishment of the kingdom of Edom.¹⁴ So we may regard the En-Gedi incident as evidence of a growing confidence of the new central authority in Edom to challenge its former suzerain and to test the strength of the Judahite defences in the southern Negev.

II Chronicles 28:16f. The Reign of Ahaz 732/31-716/15 BCE.

The second text which relates to the history of Edom's move westward is II Chronicles 28:16f. The context is the reign of Ahaz of Judah and his conflict with the Syro-Ephraimite coalition (i.e. Pekah of Israel and Rezin of Damascus) in ca. 733 BCE. The text reads: "At that time King Ahaz sent to the king of Assyria for help. For the Edomites had again invaded and defeated Judah, and carried away captives." With the word 'again' (*wd*), I assume that the Chronicler is pointing to the earlier 'invasion' of Judahite territory by the Transjordan coalition during Jehoshaphat's reign. This word 'again' is, in my view, a key word which connects II Chronicles 28 with II Chronicles 20 and suggests that Edomite westward expansion began with the attempt of the alliance of the Moabites, Ammonites and the Meunites/Men of Mt. Seir. This gives more weight to the phrase "from Edom" in II Chronicles 20:2 (see above).

At this time, despite Jehoram of Judah's best attempts to maintain his hold over the southern province with its important trade terminal of Ezion-Geber (wherever this place was located at this period),¹⁵ Edom was now an independent state with its own king (II Kings 8:20-22; the latter verse reads, "So Edom has been in revolt against the rule of Judah to this day").

At least twice during the next century, this period of independence was threatened by Judah. Once when Amaziah of Judah (ca. 793 BCE, his third year, according to Thiele¹⁶) invaded *northern* Edomite territory and defeated them in the Valley of Salt and stormed Sela (II Kings 14:7); and second,

¹⁴ "That the new kingdom of Edom began in the reign of Jehoram of Judah seems certain from I Kgs 22.47 and 2 Kgs 8.20 ...". He dates the new kingdom's appearance to the mid-840s; Bartlett 1989, pp. 118, 122. Presumably the 'old' kingdom of Edom belonged to the royal house in the time of Hadad, a contemporary of Solomon (I Kings 11:14-22).

¹⁵ There is a possibility that it is now to be located at the island of Jezirat Fara'un, about 15 km south of Tell el-Kheleifeh. See Rothenberg 1972, p. 203. It now seems that Elath was the foundation of Amaziah/Uzziah of Judah. Cf. Bartlett 1989, pp. 48, 125.

¹⁶ Thiele 1963, pp. 75, 86.

when his son, Azariah/Uzziah, occupied *southern* Edom and built or rebuilt Elath¹⁷ and restored it to Judah (II Kings 14:22 — this was probably in the decade 770-760 BCE).¹⁸ Moreover, Edom was also now coming under the influence of Assyria.¹⁹

Returning to our text, II Chronicles 28:16f, it would appear that Edom wanted to take advantage of Ahaz's political and military troubles with the Syro-Ephraimite coalition. The generally accepted reason for Ahaz's fears concerning the Syro-Ephraimite attack was his shaky hold on the Judahite throne and its succession. The aim of Rezin of Aram and Pekah of Israel was to replace Ahaz and his pro-Assyrian policy with a king favourable to their own anti-Assyrian position. That choice, it seems, was the son of Tabeel (see Isaiah 7:6) who may well have been a son of Azariah or Jotham by a princess from Tab-el, a region in the northern Transjordan.²⁰ This incident is usually placed in 734 BCE.²¹ Moreover, the name of the Edomite king taking advantage of the situation is known to us, thanks to the Assyrian records, which show that Ahaz ('Iauhazi') and Kaush-malaku of Edom were contemporaries.²² Kaush-malaku launched an attack on Judah and came away with prisoners (II Chronicles 28:17).

The corresponding passage in II Kings 16:6 reads: "At that time [i.e. while Ahaz was besieged in Jerusalem] the king of Edom recovered Elath for Edom, and drove the Judaeans from Elath; and the Edomites came to Elath, where they live to this day". It might be suggested that the Judahite settlement from which Edom took its prisoners may in fact have been Elath, built by Uzziah some 40 years earlier (see above, note 17). In which case this incident cannot strictly speaking be regarded as an invasion of Judah. Elath can only be regarded as a Judahite commercial outpost located in Edomite territory. Further, it is hard to believe that disputed territory, such as the fortified settlement of Elath,²³ would be regarded by the Chron-

¹⁷ For the archaeological reappraisal of Nelson Glueck's excavations, and the dating to the 8th-6th centuries BCE of Tell el-Kheleifeh, the site usually identified with Ezion-geber/Elath, see Pratico 1985 and 1993. Cf. also the discussion relating to it in Bartlett 1989, p. 125.

¹⁸ Bartlett 1989, p. 125.

¹⁹ In 805-3 BCE and again in 796 BCE, Adad-nirari III campaigned in the west. On one inscription from Calah he lists the peoples from whom he took tribute; Edom was among them. See Luckenbill 1926, p. 262, §739.

²⁰ See Saggs 1955, pp. 131-132. Also Albright 1955, pp. 34f. and the criticism of Mazar 1957, pp. 237f.

²¹ Saggs 1955, p. 131.

²² Luckenbill, 1926, p. 287, §801.

²³ It needs to be understood that Pratico's reassessment of Tell el-Kheleifeh discarded Nelson Glueck's view that Tell el-Kheleifeh was Ezion-geber and was Solomon's foundation. Pratico was able only to identify two main major architectural phases. The earliest one could only be assigned to the 11th-10th centuries BCE on the basis of the architectural similarity to

icler as a part of Judah, especially with his strong nationalist commitment to the Jerusalem based priestly hierarchy, nor that Ahaz would appeal to Assyria against Elath's capture by the Edomites (who were paying tribute to Assyria) as grounds for Assyrian intervention.²⁴ Seen from the Edomite point of view, the capture of Elath by Kaush-malaku was simply his way of extending his control and consolidating his power over *all* Edom. Therefore, I conclude that the attack by Edom on Judah, mentioned in II Chron. 28, was probably launched from northern Edom (usually referred to as el-Jebal).

We may envisage Edom's incursion into Judah taking place, for example, somewhere in the region of Arad, along the so-called 'way of the wilderness of Edom', over the easiest of the ancient ascents in the Judaeian desert, the Nahal Zohar. It is the only ascent in the Dead Sea scarp without a sheer cliff.²⁵

It may be appropriate at this point to refer, by way of confirmation, to Bartlett's comment on II Kings 16:6. It does help to illustrate Edomite long-term intentions. He writes:

...the capture of Elath must be regarded as an important development for the kingdom of Edom, both politically and economically. From now on Edom, not Judah, could derive the benefit of trade passing between Arabia and Damascus through the Gulf of 'Aqaba, and from now on Edom, not Judah, could control the southern region of the Wadi 'Arabah. *This made it easier for Edomites to extend their influence into the southern regions of Judah* ...²⁶

So, we can perhaps discern more clearly the significance of the events harmful to Ahaz in 734 BCE. In that year, the Assyrians under Tiglath-pileser III advanced south into Palestine and captured Philistine territory, including the Mediterranean port of Gaza. Thus Edom, we can see, had further incentive to annexe Judahite territory, namely to capture a share in the lucrative Arabian incense trade to the Mediterranean.

But, while having gained possession of its fortified settlement of Elath, the Edomites do not appear to have gained any lasting hold on the area of

the Negev highlands fortresses, but any datable diagnostic pottery was missing. The later fortified settlement was clearly datable from the wheel-made pottery forms found there to the 8th-6th centuries BCE, and from the appearance of the stamp impressions (the Qaws'anal seal) palaeographically placed in the late 7th or early 6th century BCE. See Pratico 1986, pp. 34f.

²⁴ See the references in note 18 above.

²⁵ See Aharoni 1958, p. 35.

²⁶ Bartlett 1989, pp. 127f. My emphasis.

Judah they attacked, probably, as I have said, in the region of Arad. For although there is mention, at this same time, of the towns attacked and held by the Philistines, (the following verse in II Chronicles 28:18 tells us "...the Philistines had made raids on the cities in the Shephelah and the Negeb of Judah..." i.e. the *south-west* corner of the Negev-Edom's attack would have come from the *south-east*) no such territorial success is ascribed to the Edomites during the reign of Ahaz (cp. II Chronicles 28:18).

Nevertheless, some scholars do think that the Edomites pulled off a surprise attack on the great urban centre of Tel Beer-Sheba (about 25 km west of Arad) at this time. This is the view of Nadav Na'aman, who uses the text of II Chronicles 28:17 in its support. It is part of his hypothesis that Tel Beer-sheba, level II, was not destroyed by Sennacherib's campaign in 701 BCE, but at a slightly earlier period. He insists that because of the absence at Tel Beer-sheba of the *lmlk* jar handle impressions attributed to Hezekiah, that great site could not have been targetted by the Assyrian forces; and therefore its destruction must have been the work of some other hostile force, namely, the Edomites. Moreover, "the final destruction of the towns of the Negev... and the occupation of the Valley of Beer-sheba by the Edomites were passed over in the books of Kings and Chronicles".²⁷

I Chronicles 4:34-43. The Reign of Hezekiah, 716/15-687/86 BCE.

There is an obscure piece of historical information included in the Chronicler's history which could be germane to our discussion, for here the groups we encountered in II Chronicles 20, in the reign of Jehoshaphat, again appear. I refer to the text in I Chronicles 4:34-43 which tells of the Simeonite clans who "journeyed to the entrance of Gedor ('Gerar')... in the days of King Hezekiah of Judah, and attacked their (Hamites) tents and the *Meunim* who were found there... And some of them... went to *Mount Seir* ...; they destroyed the remnant of the Amalekites that had escaped and they have lived there to this day" (NRSV). This action seems to have been the elimination by Hezekiah of the nascent Edomite satellite settlements, in order to make safe his southern borders against the attacks by Transjordan Edomites who had thrown in their lot with the Assyrians. The evidence for this scenario is the ostrakon 40 from Arad, the so-called 'Nehemyahu' letter. In this document, Gemaryahu, the commander of a fortress (possibly Ramat-Negev) situated on the road to Edom, wrote to his superior at Arad, Malkiyahu, telling him of "the evil which Edom has done". This 'evil' was

²⁷ Na'aman 1986, p. 13.

probably the capitulation of Edom to the Assyrians and the threat they and Edom now posed to the Judahite Negev.²⁸ Or, to make the event even more reprehensible, it has been suggested that the 'evil' was the destruction of the fortress of Arad level VIII by the Edomites.²⁹ Whichever was the case, Edom's tactics can be seen to be those it resorted to later in the time of the Babylonian invasion of 597 BCE, i.e. siding with an international power against Judah in order to further its own territorial aims (see p. 61 and note 44 below).³⁰ The attack on Arad is, again, evidence for Edom's constant ambition for westward expansion into the Negev.

But Hezekiah's defence preparations for his realm and his aim of restoring the ancient borders of the Davidic kingdom were to be short lived. After a period of loyalty to Assyrian, Hezekiah openly revolted in 701 BCE with disastrous results. Judah was decimated, although Jerusalem was spared. Archaeological excavations have unearthed destruction levels from a number of southern sites in Judah; for example, Lachish, Beersheba, Arad, dating to the time of Sennacherib's campaign. For the reigns of Hezekiah and the next two kings, Judah had to endure the Assyrian yoke. Edom meantime was to enter into its period of greatest prosperity under Assyria. We turn now to consider texts which concern Edom and the downfall of the Judahite state.

II Kings 24:2. The Reign of Jehoiakim 609-598 BCE.

A text which is sometimes referred to as evidence for Edomite penetration of the Judahite south is II Kings 24:2. It reads:

And the LORD sent against him bands of the Chaldeans, and bands of the Edomites, [reading thus instead of 'Aramaeans']³¹ and bands of the Moabites, and bands of the Ammonites, and sent them against Judah to destroy it, according to the word of the LORD which he spoke by his servants the prophets.

The context of the passage is the rebellion of Jehoiakim of Judah after three years of servitude to Nebuchadnezzar of Babylon. After Nebuchadnezzar's famous victory over Egypt at Carchemish in 605 BCE, he had gone on and "conquered the whole area of the Hatti-country".³² The biblical refer-

²⁸ See Anson Rainey in a note appended to Aharoni 1981, p. 74.

²⁹ Aharoni 1981, pp. 129, 149.

³⁰ Cf. Lindsay 1976, p. 25.

³¹ Malamat 1968, p. 143; Noth 1960, p. 282. See also note 30.

³² Wiseman 1956, p. 69 = (B.M. 21946, *Obv.* 1.8).

ence to the event is found in II Kings 24:7. The Babylonians soon subjugated Judah, although how long after Carchemish is disputed. Malamat thinks Jehoiakim surrendered in the autumn or winter of 603 BCE.³³

This means his tribute to Nebuchadnezzar fell due in the autumn or winter of 601 BCE. But in December of that year the Babylonian king conducted a campaign against Pharaoh Necho II of Egypt which resulted in serious losses on both sides, so that both armies had to retire to their bases in 600/599 BCE to re-equip their forces.³⁴ The Babylonian setback encouraged Jehoiakim to throw off its yoke. In Nebuchadnezzar's sixth year (599/598 BCE) his armies returned to the Hatti-land and attacked the Arab tribes of the *midbar* to the east of Riblah and Hamath, probably the Qedarites.³⁵ Then, instead of marching south with an army to deal with a difficult military situation, it would seem that Nebuchadnezzar resorted to the guerrilla group tactics mentioned in II Kings 24:2 (above) in order to keep Judah aware of his intention to return and deal with Jehoiakim's rebellion. The date, then, of the guerrilla war in II Kings 24:2 would seem to be early 598 BCE, when, according to the Babylonian Chronicle, "in the month of Adar the king returned to his own land" leaving his irregular troops to harass Judah.

Of course, it will readily be seen that the reading 'Edomites' in place of 'Syrians/Aramaeans' in II Kings 24:2 is suspect. All modern versions of the Bible have 'Syrians'. Now it is clearly the case that we can emend 'Edomites' for 'Syrians/Aramaeans' on the basis that in the *MT* the two words are sometimes miscopied by scribes, because of their similarity in Hebrew [אֲרָם, אֱדוֹם]. The grounds for scholars reading one word in preference to the other are either textual, historical or geographical or some combination of the three.

In the case of textual grounds, we have to rely on the *apparatus criticus* in the Hebrew bible. In II Kings 24:2, the evidence of the ס for reading 'Edom' is not in itself very strong, but when combined with the geographical, that is, that it is the Edomites we should expect to find with Moabites and Ammonites in a regional grouping³⁶; and with the historical, that is,

³³ See the full discussion in Malamat 1987, pp. 29of.

³⁴ See Wiseman 1956, pp. 29 and 71. Also Lipíński 1972, pp. 235-241.

³⁵ Wiseman 1956, pp. 32, 71. Cf. Jer. 49:28-33.

³⁶ Gray 1964, p. 689 and note b, writes: "Perhaps with ס we should read 'edom for 'aram as the association with Moab and Ammon indicates." Earlier expositors of the biblical texts seem to have preferred "Edom" to "Aram" on geographical grounds. For example, Burney 1903, p. 365, states that, אֲרָם rather than אֱדוֹם is to be expected in connexion with מוֹאָב and כְּנָעַי עַמּוֹן, and this emendation is favoured by the three late-19th century authorities he cites there. Cf. also the remarks of Tadmor and Cogan 1979, p. 496.

that as a nation Aram (like Israel) had been so thoroughly decimated and depopulated through deportations by Assyria at the end of the eighth century that no independent representative state remained,³⁷ then the argument for emending 'Edom' for 'Aram' becomes more cogent.

Cogan and Tadmor reject the substitution as "both textually and historically unwarranted". Possibly they believe that the textual evidence speaks for itself, but as regards the historical they state: "Edom itself may have been in a state of rebellion (Jer. 27)".³⁸ The Jeremiah reference concerns the message to the anti-Babylonian conference of envoys of Edom, Moab, Ammon, Tyre and Sidon at Jerusalem, but this passage is clearly dated to the time of Zedekiah (Jer. 27,3.12 and 28:1) between 594 and 593 BCE.³⁹ some four years later when allegiances to Babylon had changed.⁴⁰ But, we are reminded by Andersen and Freedman that "there is another consideration. Edom is always spelled *plene* [i.e. אֶדוֹם] in the Bible, a practice that requires that any confusion of Edom and Aram occur before this style of spelling came into vogue."⁴¹ It may be relevant to point out here that the Arad ostraca, discovered at that site by Aharoni in 1967 and which date to the first half of the sixth century BCE, contain at least two complete references to Edom and the spelling uses the shorter form of the name (the *brevis* form (i.e. אֶדֹם). The same can perhaps be said for the almost certain appearance of the name 'Edom' on the royal seal found on Umm el-Biyara. The second word in the second line on the seal certainly has an *aleph* followed by only two other possible characters.⁴²

There is a possible parallel reference to II Kings 24:2 in respect to the guerrilla groups. In Jeremiah 35 there appears the account of Jeremiah's dealings with the Rechabites who, in explaining the withdrawal from their nomadic way of life to become urban refugees, say:

But when Nebuchadnezzar king of Babylon came up against the land, we said, Come, and let us go to Jerusalem for fear of the army of the Chaldeans and the army of the Edomites. So we are living in Jerusalem.(v. 11).

³⁷ Cf. Pitard 1987, pp. 186-189. These are the points made by Ginsberg 1950, p. 356 and note 31. The latter is probably right when he states that Aram had ceased to exist. According to an appendix in Oded 1979, pp. 269f., the Assyrian kings between ca. 745 and 701 BCE visited Bit-Hazaili at least five times and deported captives.

³⁸ Cogan and Tadmor 1988, p. 306.

³⁹ Cf. Malamat 1987, p. 296 and see below p. 11.

⁴⁰ Because Jeremiah (28:1) places the conference in "the fourth year" of Zedekiah but describes it in an unusual manner, Sarna believes that the time of the conference relates not to the reign of the king but to the fourth year of the sabbatical cycle, and consequently pushes the conference back to 597 BCE; Sarna 1978, p.96. His argument, however, is not convincing enough to have won widespread approval.

⁴¹ Andersen and Freedman 1989, p. 259.

⁴² Bennett 1966, p. 399.

Here, again, the reading according to the MT is 'Aramaeans/Syrians' with the margin reading 'Edom' (ס), or even with ג reading 'Assyrians') which in effect does little to elucidate the problem.⁴³ The question clearly needs further consideration and I have therefore reserved that for an excursus at the conclusion of this paper.

The evidence of the Arad ostrakon no. 24, which implies an Edomite attack on the Judahite Negev, I believe also fits better the circumstances of the guerrilla warfare in 598 BCE than in the later period of Jerusalem's destruction in 586 BCE.⁴⁴ Aharoni has stated this latter position since publishing his first account of the Arad ostraca.⁴⁵ His reason for doing so is not entirely clear. But it could perhaps be to accommodate his hypothesis about Inscription 20 which mentions "in the third year" and which he believes relates to the reign of Zedekiah.⁴⁶

II Kings 24:8-17. The Reign of Zedekiah 597-586 BCE.

Late in 598/7 BCE, Nebuchadrezzar marshalled his forces and came up against Judah and Jehoiakim to deal with the rebellious vassal. Jehoiakim died and was succeeded by Jehoiachin before the siege of Jerusalem had begun (II Kings 24:6). According to Wiseman, the siege could not have lasted more than two months.⁴⁷ Jehoiachin surrendered the city on the second of Adar (March 16) 597 BCE and the first deportations took place. Nebuchadrezzar "appointed a king of his own choice, received its heavy tribute and sent (them) to Babylon" (Bab. Chronicle, BM 21946 rev. l 13). Zedekiah began his 11 year reign as Nebuchadnezzar's vassal.

What was the effect of the conquest of Jerusalem in 597 BCE on the rulers of Edom and Judah's other neighbours? It would appear that there was consternation at the deportation of the cream of the population; the king and royal family, priests, prophets, high officials, the upper class as a whole, the chief military personnel and the best of the artisans and army technicians: the "good figs" of Jeremiah 24:1-7. This must have alarmed them exceedingly. Consequently, we see the flowering within three or four years of an anti-Babylonian rebellion.

⁴³ Compare the assessment of the historical worth of the passage in McKane 1988, pp. 118-119.

⁴⁴ See the position I argued in Lindsay 1976, p. 25 and which, I think, is still valid.

⁴⁵ Aharoni 1969, pp. 10-21.

⁴⁶ Cf. the criticism of Aharoni on this point in Malamat 1987, p. 307, note 40.

⁴⁷ Wiseman 1956, p. 33.

Jeremiah 27 records the conference of envoys from Edom, Moab, Ammon, Tyre and Sidon at Jerusalem. This is dated to the fourth year of Zedekiah (i.e. 594/3 BCE—Jer. 27:1, 3, 12; 28:1). Here we have an apparent reversal of Edom's attitude towards Judah. In 598 BCE Edom had assisted Babylon in subduing the rebel Judah by harassing its territory and population. Now it sends an envoy to the meeting of conspirators. Why?

Hopes must have been high in Edom and in the other states for a return to independence and the reasons were, first, there was a resurgent Egypt waiting to return to the forefront of Palestinian affairs (an Egyptian army was able to come to the assistance of Judah during the final siege of Jerusalem, *cf.* Jer. 37:11); and, second, in the years immediately following the surrender of Jerusalem, Nebuchadrezzar was entirely preoccupied with a military revolt in his own capital and with attacks from Elam. These problems in Babylon seemed to harbingers a retreat of Babylonian influence and encouraged the spirit of resistance to its power in the states of the Hatti-land.⁴⁸ Third, Edom doubtless perceived that, for the present, there was more to be gained from political unity and consensus with the Transjordanian states than from taking sides with Babylon and playing one state against another in the interests of survival.

But, as it turned out, the conspiracy achieved nothing and the movement petered out. No coalition of forces emerged from the Jerusalem conference to face the might of Babylon. John T. Strong has argued that one reason for the breakdown in the conference was a decision by Tyre to adopt a neutral or isolationist position in relation to Babylon.⁴⁹ His analysis of the oracles in Ezekiel 26-28 leads him to believe that Tyre decided to trust in its geographically superior position as an island city when facing the threat of Babylon rather than in a system of alliances. Further, he believes that attitude is reflected in the oracle of Amos against Tyre (Amos 1:9f.) where Tyre is accused of breaking the covenant of brotherhood with Judah (i.e. at the Jerusalem conference there was 'treaty between allies'; in Near Eastern treaty language they were 'brothers'). When he raises the question of why did these treaty partners jump ship, his explanation seems to boil down to this: that there was a lack of prophetic support (Jeremiah) for Zedekiah and that Nebuchadnezzar's success in quelling a revolt in his army boded ill for the conspirators!⁵⁰ Clearly, apart from Judah and possibly Ammon, the political will to take on Babylon was just not strong enough.

⁴⁸ *Cf.* Malamat 1987, p. 296.

⁴⁹ Strong 1997, 207-219.

⁵⁰ Strong 1997, p. 218.

It would seem then that, as a result, Edom followed suit and did the same. Taking advantage of a rapidly changing situation, the Judahite south was attacked by Edomite troops advancing against the fortress of Ramat-Negeb (Horvat 'Uzza, about nine km south-south-east of Arad) the chief Judahite fortress in the area. Both these centres were destroyed by the Edomites in 595/4 BCE. This is the scenario suggested by the ostraca from Arad attributed to level VI of that site. Ostrakon no. 24 reads:

From Arad 50 and from Kin[ah]...

And you shall send them to Ramat-Negeb by the hand of Malkiyahu the son of Qerab'ur and he shall hand them over to Elisha' the son of Yirmiyahu in Ramat-Negeb, lest anything should happen to the city. And the word of the king is incumbent upon you for your very life! Behold, I have sent to warn you today: [Get] the men to Elisha': Lest Edom should come there.

The message on the ostrakon is probably to be dated to the third year of Zedekiah (595/4 BCE), to judge from another one (no. 20), on a jar also from level VI, inscribed with the words: "In the third year, (in the) month of Sah." Aharoni believes that this inscription (no. 20) contains a reference to the reign of Zedekiah and that the two ostraca relate to the last days of the Arad citadel before it fell to the Edomites in the third or fourth year of that king.⁵¹

II Kings 25:1-21 records Judah's final two years and Zedekiah's second attempt to break free of the Babylonian yoke. His first attempt seven years previously had involved, as we have seen, the Transjordan states and the ports of Tyre and Sidon. Toward the end of his ninth year (589/8 BCE), he rebelled. Consequently, Nebuchadrezzar began his offensive. The Babylonian siege of Jerusalem took place on the 10th Tebet of Zedekiah's 10th year (15 January 588 BCE: II Kings 25:1; Jer. 39:1, 52:4). Previously, the political will had been lacking in Zedekiah. What factors had now made him change his mind and think the time was propitious?

One factor clearly was the turbulent state of affairs generally in the west. The area was certainly difficult for Babylon to control (there was no *Pax Babylonica*), as indicated by the fact that after the initial conquest of the Hatti-land, the Babylonian Chronicle mentions 10 visits in 13 years (605/4-594 BCE) by the Babylon armies to maintain a hold on their conquests.⁵²

Another was the continual hope of Egyptian support and intervention to counter Babylon's hegemony in Palestine. We may infer from the account

⁵¹ For the reconstruction just given, see Aharoni 1981, pp. 40f., 150f., 182. Note that in the dating I have followed Thiele 1962, p. 170.

⁵² See Wiseman 1956, pp. 69-75.

of the failed conspiracy of Jer. 27 that Zedekiah would turn to the only other Near Eastern power which had any success against Babylon and Egypt (see Ezek. 17:13-15). Jer. 37:5 tells us that the siege was, indeed, temporarily lifted as the result of the appearance of an Egyptian army, an event, according to Freedy and Redford, took place somewhere in the vicinity of August-September, 588 BCE.⁵³ Malamat prefers to date it in the spring of 587 BCE.⁵⁴ At that time, it is possible that some of the citizens of Jerusalem, perhaps discerning the writing on the wall, escaped to take refuge in the Transjordan states of Edom, Moab and Ammon (cf. Jer. 40:11).

Some scholars make much of Edom's acceptance of Jewish refugees who fled to Edom.⁵⁵ They tend to interpret Edom's actions as a gesture of a friendly neighbour in the light of the Jerusalem conference of 594/3 BCE; but equally, it needs to be remembered, that it could be interpreted as political opportunism on Edom's part. When the refugees began to arrive in 588 BCE (and later), it signified for Edom that Judah was finished and that the way now lay open for a further move against Judahite territory.

This brings us to the final stage of describing Edom's forays into the Judahite south, using the biblical texts. We shall examine Edom's part in the destruction of Jerusalem and the overthrow of the Jewish state between 588 and 586 BCE, for there, too, there is some evidence of Edomite expansionism.

Texts in the Prophets

There are certain biblical texts which offer *prima facie* evidence of Edom's complicity in Judah's downfall, and thus imply Edom's aim to occupy Judahite land. One of the most explicit is found in Obadiah 10-14.

For the *violence* done to your brother Jacob, shame shall cover you, and you shall be cut off for ever.

On the day that you *stood aloof*, on the day that strangers carried off his wealth, and foreigners entered his gates and cast lots for Jerusalem, you were like one of them.

But you should not have gloated over the day of your brother in the day of his misfortune; you should not have rejoiced over the people of Judah in the day of their ruin; you should not have boasted in the day of distress.

⁵³ Freedy and Redford 1970, p. 471. Cf. Malamat 1987, p. 300.

⁵⁴ Malamat 1987, p. 301 and note 47.

⁵⁵ E.g. ben Zvi 1996, p. 236 and note 22.

You should not have *entered the gate* of my people in the day of his calamity; you should not have gloated over his disaster in the day of his calamity; you should not have *looted* his goods in the day of his calamity.
 You should not have stood at the parting of the ways to *cut off his fugitives*; you should not have *delivered up his survivors* in the day of distress. (My emphasis)

J.R. Bartlett has examined these texts and others in the prophetic material on Edom and tends to regard most of them with suspicion. “Hard evidence for Edom’s behaviour when the Babylonians finally attacked and besieged Jerusalem in 589 BCE”, he writes, “is less easy to find than is often supposed.” He believes that most of these passages “picture Edom’s attitude rather than her actions”. He concludes that “in fact, Edom played no direct part in the events of 587 BCE... For the destruction of Jerusalem and Judah in 587 BCE Edom cannot be held responsible.”⁵⁶

I find his point of view difficult to accept. ‘Attitudes’ are expressed by either words or deeds or both. Looking at the extracts above, I would say that Edom *acted* as well as spoke out against Judah. “Violence done”, for example in Obadiah, is surely more than a wrong attitude on Edom’s part, even though the precise form it took is not stated. Similar sorts of criticism could also be made concerning the other emphasised texts above. To have “entered the gate” (of Jerusalem), “looted”, and “cut off fugitives” are all quite explicit acts, as Bartlett himself virtually concedes. He states, of Obadiah, “Verses 12-14... appear to suggest that Edom’s part was much *more active*... The charges of cutting off fugitives and delivering up survivors *could have some foundation*... The charges in v. 13, however, of entering the gate of God’s people and looting are *more serious*”. This is more than appearing to “pick up the description of Babylonian activity in v. 11” or Edom being “*pictured as* behaving just as Babylon behaved”.⁵⁷ Edom, I would contend, *did* copy Babylon’s behaviour and engaged in similar acts because it had allied itself with Babylon, and should not merely be “credited with Babylon’s behaviour”, as he asserts.

The reason Bartlett adopts the position he does seems to stem from his view of the nature of the material he is discussing. “... Obadiah should not be understood as an *historian’s* description of Edom’s behaviour in 587 BCE. The *poet* derives his picture largely from his imagination”.⁵⁸ But the formal structures of Hebrew prosody are capable of conveying historical facts/truth.

⁵⁶ Bartlett 1989, pp. 151, 157.

⁵⁷ Bartlett 1989, p. 154. My emphasis.

⁵⁸ Bartlett 1989, p. 155.

However, it has to be allowed that much depends on the weight one gives to the meaning conveyed by the English translation of the key verses in Obadiah 12-14. For example, the difference from a tense perspective between “You should not have entered the gate of my people” (*RSV/NRSV*), and “Do not... enter my people’s gates” (*NEB/REB*) is patent. The first rendering makes the action *retrospective*; the second, *prospective*. The first has the action seen from a *future* standpoint, looking back on the “entering”; in the second, it is seen from the standpoint of the *past*, giving a warning about a future possible “entering”. The *RSV/NRSV* version of the English bible is regarded as a literal translation (“the Committee has followed the maxim ‘As literal as possible, as free as necessary’”).⁵⁹ The *NEB/REB* version, is regarded more as a sense-for-sense translation (“in the Revised English Bible, the guiding principle adopted has been to seek a fluent and idiomatic way of expressing biblical writing in contemporary English”).⁶⁰ The first rendering tends to support the idea that Edom was involved in the Babylonian destruction of Jerusalem. The second tends to support the idea that Edom may have heeded a prophetic warning (and abstained) and so may be acquitted of having taken part in the overthrow of the city. Can we resolve the dilemma? Can linguistic or text analysis help?

In his commentary on Obadiah, Paul Raabe has pointed out that “verses 12-14 consist of a series of eight bicola or line pairs that speak of Edom’s anti-Judahite hostilities”. Since vv. 10-11 indicate that the prophet speaks from a historical time *subsequent* to these hostilities, one naturally understands vv. 12-14 to refer to past events. Therefore some authorities translate the syntagm *’al* + second person jussive, which initiates each of the eight bicola, with *past* subjunctives in English.⁶¹ Hence, the RSV rendering “You should not have gloated...”.

However, he goes on to make the observation (credited to C.R. Krahmalkov) that “To express a past subjunctive — ‘you should not have’ — Hebrew uses *lmh* + perfect ‘Why did you gloat?’ [So that] the construction used in vv. 12-14, *’al* + second person jussive, is the standard way to make a vetitive or negative command, as Hebrew does not negate the imperative form. Therefore it should be translated ‘Do not do so-and-so’ (so LXX, Vulgate, Peshitta, NEB, NASB)... By definition vetitives concern present or future time.... They can be rendered ‘Do not begin an action’... or ‘Stop doing an action’... The context dictates.”⁶²

⁵⁹ From the translators’ preface 1991, p. xii.

⁶⁰ From the translators’ introduction 1989, p. xvii.

⁶¹ Raabe 1996, pp. 176f. My emphases.

⁶² Raabe 1996, p. 177.

So, what understanding *inter alia* does Raabe come to about the verbal clauses in vv. 12-14? He sees the prophet Obadiah exhorting Edom (a) “not to engage in anti-Judahite behavior”, and (b) “to stop engaging in these kind of hostilities.”⁶³ If I understand him aright, the first exhortation is an explanation of the text if we imagine that the prophet has projected the past catastrophe into the present. The second is one in which we imagine the prophet has located himself back in time to the period of Judah’s fall. Note that this imagining of the prophet is not the same kind of imagining as that envisaged by Bartlett (p. 65 above).

The closely argued text analysis by Ehud ben Zvi is somewhat similar. He states that the Hebrew verbal form used in the text, *’al* plus a verb in the jussive and in the second person “may be understood as (I) ‘do not “do”...,’ or ‘you should/ought/must/will not “do”, but also as (II) ‘you should not have “done”’.⁶⁴ The meaning evidently depends on the standpoint of the readers (the original ones or later).

Thus, he goes on to write that “the more ambiguous the time frame within the book is, and the more multiple (re)readings are evoked by the text, the weaker is the case for maintaining that the text accurately, and unequivocally reflects historical events”. But for him the ‘historical events’ are the “claims that the text here accurately reflects an actual (political) warning [given] to Edom, just before the events of 586 BCE...”.⁶⁵

What can be concluded from the text analysis of these two scholars? On the one hand, ben Zvi chooses to adopt Bartlett’s position when it comes to deciding the historical question of whether Edom had a hand in Jerusalem and Judah’s overthrow — “As for Edom’s actions at or around the time of the conquest of Jerusalem, there is no solid evidence that Edom played then any major, hostile role.” And this followed by a footnote, “In this regard, one is to follow the critical evaluation of the evidence offered by Bartlett (1989: 151-57)”.⁶⁶

On the other hand, Raabe states “The book [Obadiah] accuses the Edomites of taking advantage of the crisis within Judah. According to vv. 12-14, at the time of Judah’s ruin they gloated, stole property, (cf. v. 5), and captured fugitives. Unlike other parts of the book that employ figurative language and imagery, these verses use realistic language and make specific accusations. Only if the Edomites actually engaged in such activities does the prophet’s expression of shock and disbelief make sense. Therefore we should understand vv 12-14 as an accurate description of Edom’s behaviour

⁶³ Raabe 1996, p. 177.

⁶⁴ ben Zvi 1996, p. 145.

⁶⁵ ben Zvi 1996, p. 145f.

⁶⁶ ben Zvi 1996, p. 236 and note 22.

rather than as the prophet's poetic imagination (*pace* Bartlett 1989: 154-55).⁶⁷

So, it seems we are reduced to this: that one's preference in the light of the textual evidence is circumscribed by one's views/assumptions. Ben Zvi is concerned to maintain primarily the prophetic/theological character of the book of Obadiah written for an educated literati in later (Achaemenid) times. Consequently, the book has a certain ambiguity about it: 'Edom' is sometimes regarded as an entity, an individual state and people, but at the same time 'Edom' can also be a codeword for the 'nation', many of which were condemned by the prophets; but, of the nations, Edom was the only one singled out because of its traditional 'brother' role to Israel/Judah. Such an interpretation requires that the message of the book of Obadiah should not be related to any one historical period or confined to the historical Edom.⁶⁸

Raabe, by contrast, is concerned to stress that the book is grounded in the events that overtook Judah in *the* primary happening of its history and people. He believes strongly that to attach too much importance to the book as a prophetic oracle with its emphases on conventional language and stereotypical patterns is wrong. It pits conventionality against history. Consequently, Raabe prefers to aim for specificity in time and geographical setting.⁶⁹ I, for one, would concur with his remarks cited above concerning Obadiah's historical worth, for if the Edomites did not actually engage in such activities, then the prophet's expression of outrage does not make good sense.

Raabe, in his view of the date of the book of Obadiah, believes it best fits in the first half of the exilic period, namely, ca. 585-555 BCE; "the *terminus a quo* is the fall of Judah and Jerusalem to the Babylonians (587/6), while the *terminus ad quem* is the campaign of Nabonidus against Edom (553)".⁷⁰

We turn now from Obadiah to the book Ezekiel to examine the important verses in chapters 35 and 36.

Ezek. 35:5

Because you cherished perpetual enmity, and *gave over the people of Israel to the power of the sword* at the time of their calamity, at the time of their final punishment;

⁶⁷ Raabe 1996, p. 52.

⁶⁸ Cf. the concluding remarks, ben Zvi 1996, pp. 253ff.

⁶⁹ Cf. the remarks of Raabe 1996, pp. 47f.

⁷⁰ Raabe 1996, p. 51.

Ezek. 36:5.

...therefore thus says the Lord GOD: I speak in my hot jealousy against the rest of the nations, and against all Edom, who *gave my land to themselves as a possession* with wholehearted joy and utter contempt, that they might possess it and plunder it. (My emphasis)

Ezekiel prophesied to the exiles in Babylonia between July, 593 BCE (the fifth year of Jehoiachin, Ezek. 1:2) and 563 BCE, if the puzzling '30th year' of 1:1 is to be taken as a reference to the date of the prophet's call. I would continue to maintain the historical worth of references here to Edom; as in Obadiah, so in Ezekiel.⁷¹ As Raabe has pointed out, the accusations against Edom in Obadiah are the same as those given in Ezekiel (rejoicing and gloating over the downfall of Judah, the giving over to the sword of Judahite people and the desire to occupy Judahite lands).⁷²

There is but one chief oracle against Edom in Ezekiel, and that is chapter 35, vv. 1 to 15. The first point to notice about this oracle is that it is probably an editorial expansion of the three verses devoted to Edom in the section of the book, *Oracles Against Foreign Nations*, chapters 25-32. But editorially it has been placed alongside the prophecy about the restoration of the 'mountains of Israel' in chapter 36 by way of contrast (compare the reference to the 'mountains of Israel' in the oracle against Edom in chapter 35:12).⁷³ In chapter 36 Edom is singled out with "rest of the nations" who "took my land as their possession" as if to emphasise Edom's deliberate intention to take over the Judahite south.

A second point to notice about the oracle in chapter 35 is that Edom is described under its westward toponym, Mt Seir. This suggests that the base for any Edomite move on the Judahite Negev was the Edomite territory west of the Arabah. The text of Ezekiel 35 mentions settlements of Mt Seir (vv. 4 and 9). I think it significant that only here in Ezekiel 35 and 36 (apart from one other use in II Samuel 8:14 where the extent of David's conquests are being described) in the Hebrew bible does the term 'all Edom' occur, viz. "... you shall be desolate, Mount Seir, and *all Edom*, all of it" (35:15); and "... I am speaking in my hot jealousy against the rest of the nations, and against *all Edom*, ..." (36:5). The implication of this use of the term at this time is that Edom's territorial borders also included Mt Seir, to the *west* of the Arabah. It also seems that in this period there existed among the peoples of ancient Near Eastern states a concept of relatively precise national

⁷¹ Our confidence in the system of dates in Ezekiel has been strengthened since Freedy and Redford's study. See note 45.

⁷² Raabe 1996, p. 52.

⁷³ Cf. Wevers 1969, pp. 264f; Zimmerli 1983, p. 232.

boundaries.⁷⁴ This would be the case when trade routes and mining access rights were important and perhaps were being called into question. Mention of Mt Seir also highlights Edom's interest in the Arabah where border concerns and tensions might exist. Judges 1:36 (supply 'Edomites' for 'Amorites'⁷⁵) mentions the common boundary/border/frontier (*g'bûl*) between Judah and Edom. It is a text which has been described as a post-exilic gloss:⁷⁶ a view and explanation involving biblical chronology which has not changed much in the last half century.

Nelson Glueck in an early study of the boundaries of Edom set out to show emphatically that "the territory of Edom never extended west of the Arabah" and categorised most of the relevant texts which gave this impression as 'post exilic glosses'⁷⁷, but more recent studies would go further than merely confirming his view. Scholars who adopt a modern sociological approach to biblical literature conclude that the vast majority of texts which make up the so-called history of Israel are a late construct of the biblical community of post-exilic Yehud and date to the end of the sixth and early fifth centuries BCE.⁷⁸

Texts in the Apocrypha

When it comes to accounting for the rancour felt towards Edom by the writers in our textual sources (e.g. Ezekiel, Obadiah) inevitably there are questions when the nature of Edom's offences are mentioned. Bartlett points out persistently that the charges levelled against Edom in these books are "general and conventional rather than specific and circumstantial".⁷⁹ This not entirely true, as I have sought to show in discussing the book of Obadiah. In any case there are other sources which can be used to support the specificity of the charges.

There are references in the book of First Esdras which we can consider because they state explicitly what were probably the chief reasons for the strong anti-Edomite feeling felt by the exilic and post-exilic community in Judah. The texts from First Esdras are as follows.

⁷⁴ See Grosby 1997, pp. 1-29, especially pp. 4-6, where he discusses Edom.

⁷⁵ Cf. Burney 1918, p. 33.

⁷⁶ Boling 1975, p. 61.

⁷⁷ Glueck 1936, pp. 153f.

⁷⁸ See, for example, the two volumes produced by the Sheffield based Journal for the Study of the Old Testament: Davies 1991 and Eskenazi & Richards 1994. For a clear introductory statement of the position see Davies 1991, pp. 11-19.

⁷⁹ Bartlett 1989, pp. 153.

First Esdras 4:45

You [King Darius] also vowed to build the temple, which the Edomites burned when Judea was laid waste by the Chaldeans.

First Esdras 4:49-50

He [King Darius] wrote on behalf of all the Jews who were going up from his kingdom to Judea,... that all the country that they would occupy should be theirs without tribute; that the Idumeans [i.e. Edomites] should give up the villages of the Jews that they held ...

First Esdras 8:68-69

After these things had been done, the leaders came to me and said, 'The people of Israel and the rulers and the priests and the Levites have not put away from themselves the alien peoples of the land and their pollutions, the Canaanites, the Hittites, the Perizzites, the Jebusites, the Moabites, the Egyptians, and the Edomites.'

These texts are worthy of closer attention than is usually accorded them when it comes to the matter of Edom's involvement in the overthrow of the Judahite state. They make some singular claims: Edomites had a hand in burning the Jewish temple; they took over Jewish villages; religiously, they were in part a cause of the ritual contamination of the Jewish people.

A point that stands out concerning the significance of the first of these texts, about the Edomites burning the Jerusalem temple, is this: it would go a long way to explaining the strength of anti-Edomite feeling in the biblical texts considered above. Add to it the second, about the Edomite occupation of Jewish settlements in Judah, and we have what would appear to be a very good case indeed. But, of course, these texts have already been looked at concerning Edomite-Judahite relations. Bartlett has discussed them in his *Edom and the Edomites*, pages 155f., where he evidently considers them too late chronologically (third or second century BCE) to be of any consequence, or that they are contradictory, in that other biblical texts, II Kings 25:8-9, II Chronicles 36:17-19, Jeremiah 52:12-13, ascribe the burning of the temple to the Babylonians. But I think that the matter warrants further consideration.

To begin with, it is not altogether inconsistent to believe that the statement about the burning of the temple in the biblical texts and First Esdras could both be true, that together the Babylonians *and* the Edomites were responsible for burning the temple. That in fact, as I have argued elsewhere⁸⁰, the Edomites acted in concert with Babylon. And while the Babylonians were distant enemies, the Edomites were Judah's geographical neighbours who occasionally, from whatever motives, allied themselves with

⁸⁰ Lindsay 1976, p. 29.

Judah (see pp. 64-65 above). But they were the ones who, it could be argued, benefited most from Judah's calamities. Indeed, as they stand the biblical texts are somewhat ambiguous about who was responsible for burning the temple. In II Chronicles 36, for example, there is a description of the event in question. After a series of singular verbs in vv. 17 and 18, which read: "17. Therefore he brought up against them the king of the Chaldeans, who killed their youths with the sword in the house of their sanctuary, and had no compassion on young man or young woman, the aged or the feeble; he gave them all into his hand. 18. All the vessels of the house of God, large and small, and the treasures of the house of the LORD, and the treasures of the king and of his officials, all these he brought to Babylon. 19. They burned the house of God, broke down the wall of Jerusalem, burned all its palaces with fire, and destroyed all its precious vessels" (NRSV), verse 19 of chapter 36 introduces a plural verb ("And *they* burned the house of God"). The subjects of the verbs in vv. 17 and 18 are the king of Babylon and Yahweh respectively. It would be possible to interpret the subjects of the plural verb 'they burned' as these same two, but that, of course, appears unlikely. So, perhaps it is acting a little too hastily to have the Chronicler 'unambiguously' ascribing the burning of the temple to the Babylonians (*pace* Bartlett). We could perhaps conjecture that the Edomites were collaborators with the Babylonians.

In the text of II Kings 25:8ff., the burning of the temple is ascribed to the Chaldean, Nebuzaradan, the captain of the bodyguard, a servant of Nebuchadnezzar (Jeremiah 52:13 duplicates this, as does Josephus — Ant. X.8.5). So, of course, the 'they' in Chronicles 36:19 could be a reference to the king of Babylon's agent, Nebuzaradan. Or perhaps, more naturally, the army of the Babylonians, including detachments of Edomites, as *other* agents of Nebuchadnezzar.

It could perhaps be significant that the parallel passage to II Chronicles 36:17 in First Esdras (1:52) reads "...he (the God of their fathers) gave command to bring against them the *kings* of the Chaldeans." This phrase "*kings* of the Chaldeans" could be interpreted to mean 'kings *subject to* the Chaldeans', a reference to those national groups whose kings assisted Babylon in 598 BCE, the period of Jehoiakim's rebellion (II Kings 24:2; see p. 8 above, "bands of the Chaldeans, and bands of the Edomites..."). In other words, it could have been the Edomites. Perhaps this would explain why the author of First Esdras uses the plural 'kings'. As far as we know, Nebuchadnezzar (605-562 BCE) was the only Babylonian king involved in the events, from the battle of Carchemish (605 BCE) onward, that led to the downfall of the Judahite state. Nabopolas-

sar, the father of Nebuchadrezzar, certainly played no part in it (he died in 605 BCE).⁸¹

But let us take a closer look at the texts from First Esdras chapters 4 and 8. In 1910 Charles Cutler Torrey published his influential *Ezra Studies* in which he considered the nature and origin of this apocryphal work.⁸² His detailed analysis of its text is still very useful. Jacob Myers' 1974 commentary on Esdras noted that, after consulting the works of Moulton (1899), Bayer (1911), Walde (1913) and Torrey (1907, 1910, 1945), little could be added to the discussion of the book's textual and historical significance. He paid tribute to Torrey's work and observed that even in his revised estimate of it, Torrey still retained his original views. Myers also remarked that although Torrey's work deserved study and consideration it was "hardly as fool-proof as he seemed to think". Despite this comment, he gave no reasons for his evaluation.⁸³ Torrey's thesis on First Esdras is complex and it may have seemed to Myers that it required a disproportionate amount of space to consider it in detail.

Earlier, W.F. Albright and H.L. Ginsberg had shown a decided regard for Torrey's work. Ginsberg has cited approvingly a letter from Albright dated 13 November 1947 in which the latter stated concerning Edomite encroachment into the Negev that "I Esdras 4:50, which has been shown pretty convincingly by Torrey to belong to the original of the Chronicler's work, states that the Edomites had occupied Jewish villages before the outset of the reign of Darius."⁸⁴ Albright was persuaded that Torrey's researches had been able to demonstrate that embedded in chapters 4 and 5 of First Esdras was a lost section of the Chronicler's history belonging to our canonical book of Ezra. The lost part embedded in First Esdras consisted of First Esdras 4:47b-56; 62-63 and 5:1-6. According to Torrey, in our canonical book of Ezra, this section originally followed the last verse (v. 11) of Ezra chapter 1. His view of First Esdras is that it is simply an unchanged extract from an older Greek ('Septuagint') of Chronicles-Ezra-Nehemiah known to have been in existence in the middle of the second century BCE.⁸⁵ He argued that our canonical text of Ezra-Nehemiah was made in the second century CE; it does not give us all that the Chronicler wrote, but it can be restored from the text of First Esdras.

What makes Torrey's textual analysis of First Esdras and its relationship to canonical Ezra credible is that, although a necessarily complicated expla-

⁸¹ See Wiseman 1961, p. 26.

⁸² Torrey 1910 and 1970 and also especially chapter 2, previously published in Torrey 1907, 116-141.

⁸³ See Myers 1974, pp. 14f.

⁸⁴ H.L. Ginsberg 1950, p. 364, note 47a.

⁸⁵ See the later more concise account of his work in Torrey 1945, p. 45.

nation, it is a coherent and generally plausible account. Anyone who takes the trouble to work through it and the methodology behind it will find it illuminating and usually persuasive. His explanation of the excision of the Chronicler's history and how it came about appears to fit the data and does justice to the evidence relating to the history of the LXX and the MT in their various recensions. Torrey above all had the linguistic competence to handle the textual problems. Where there are obscurities in his account, and there are some, they do not create insuperable difficulties for the integrity of his thesis. It perhaps needs to be remembered that Torrey was a radical scholar when it came to historical criticism. As late as 1954 he still held Ezra to be an unhistorical figure and dated the Chronicler's work to ca. 250 BCE.⁸⁶

But what about the important historical statement in First Esdras 4:45, that the Edomites burned the temple? This lies outside the 'lost part' of the Chronicler's work, according to Torrey's analysis, so, how does he regard this verse? His view is that the verse is part of the work of an interpolator who, about 200 BCE, inserted the Story of the Three Youths into the Chronicler's history and then had to use an appropriate narrative section, which Torrey calls a 'patch', to effect the transition from the end of the Story of the Three Youths to the ('lost') Chronicler's account of the preparation for the Jewish exiles return to Jerusalem. This section of narrative, or expansion, consists of First Esdras 4:43-46, with its statement about the Edomites burning the temple.⁸⁷

Torrey describes the interpolator as skilful and adaptive. "The insertion", he writes, "of such a foreign sounding episode would require a formal connection, on either side [i.e. at the beginning and ending of the insertion], both firm and plausible." The interpolator's editorial work, Torrey later adds was "admirable in effectiveness and in economy of material. In the case of each of these insertions the connection on either side is perfect and the transition wholly natural. This was a redactor of more than ordinary ability."⁸⁸ This capable redactor, then, is the one who added the information about Edom's part in the burning of the temple. So, what can be deduced about him, and how historically accurate is his information likely to be?

Apart from his skill as a redactor, it is probably true to say that he was Jewish. Although the Story of the Three Youths is characteristically a piece

⁸⁶ See Stinespring's prolegomenon in Torrey 1970, pp. xi-xxviii.

⁸⁷ Torrey 1945, pp. 50f. Cf. Torrey 1970, p. 28.

⁸⁸ Torrey 1945, pp. 50 and 52.

of pagan literature,⁸⁹ it is clear from the use to which the story has been put in the Chronicler's history that the interpolator has adapted it for Jewish interests. Torrey has pointed out that, in the story where the third contestant makes women the strongest, this outcome would hardly satisfy a devout Jewish point of view. So, "the interpolator solved the problem excellently by adding the praise of Truth and including in it a casual mention of the one supreme God (4:35-40 last clause)".⁹⁰ But just why a Jewish writer would want to include this story in the Chronicler's History in the first place Torrey never really makes clear. The closest he comes to an explanation is the statement that "The plan of the interpolator [was] to make the restoration of Israel a joint project of Darius and Cyrus...".⁹¹ This is a reference, according to Torrey, to the interpolator's particular view of Medo-Persian historiography, namely that Darius Hystaspis, as 'Darius the Mede', was believed to have preceded Cyrus.

Research on First Esdras has, of course, not stood still since Torrey's day and scholars in this field today hold differing views on the nature of First Esdras.⁹² Consequently, trying to use this source for historiographical purposes or for historical reconstruction is difficult. But the influence of Torrey's studies should not be written off just yet.⁹³

To summarise this paper, the biblical texts can be used to illustrate the increasing intensity of Edomite expansionism between the early ninth and early sixth centuries BCE. While its initial push during the reign of Jehoshaphat of Judah netted it no tangible gains, it encouraged Edom to seek out more powerful allies and in the following century it gained a foothold in Judahite territory west of the Arabah in the Assyrian period. But this achievement was short lived and the meagre settlements it had founded were destroyed in the time of Hezekiah. But in the early sixth century BCE, having sided with Babylon, Edom successfully attacked and probably held sites in the Negev such as Arad. In less than a decade, Edom accomplished its long term ambition when it assisted Babylon in the overthrow of the Judahite state and then established settlements in the Negev.

⁸⁹ Torrey 1945, p. 50.

⁹⁰ Torrey 1945, p. 54.

⁹¹ Torrey 1945, p. 52.

⁹² See especially Williamson 1996, pp. 201-216; Schürer 1987, pp. 708-718; Eskenazi 1999, 344-347.

⁹³ Cf. the remarks of Grabbe 1991, 98f.

Excursus on II Kings 24:2: on reading 'Edom' for 'Aram' in MT.

This text is important enough to devote an excursus to it in order to present the supporting evidence for preferring 'Edom' to 'Aram'. That the possibility exists for emendation here is well known. As David Clines observes:

There are of course cases in our printed texts of the Hebrew Bible where emendation is unquestionably required; for example, there are several cases where אֲדוֹם, Edom, is undoubtedly a scribal error for אֲרָם, Aram, and vice versa.⁹⁴

The text of II Kings 24:2 in *BH Stuttgartensia* lists as Judah's Babylonian-inspired attackers Chaldeans, Arameans, Moabites and Ammonites, in that order.

On a negative note, regrettably there are no documents or fragments of documents of II Kings among the texts from Qumran with which to compare the *MT*. The latest publication of *Discoveries in the Judaean Desert* has only some fragments of I Kings 7 and 8.⁹⁵

The *Septuagint*⁹⁶ which gives a reconstructed critical text, has Chaldeans, Syrians [Aram], Moab and Ammon. The *Peshitta* version of the Old Testament in Syriac⁹⁷ reads Chaldaeans, Edom, Moab and Ammon. The Syriac Hexapla of Origen⁹⁸ gives Syrians [Aram], Moabites and Ammonites. The *Vulgate*⁹⁹ supplies Chaldaeans, Syrians [Aram], Moab and Ammon. All these lists of names occur in the order given here.

Clearly, from the textual evidence alone, the reading 'Aram/Syrians', rather than 'Edom', carries more weight. But this is a mechanical kind of method to determine the accuracy of an account of a purportedly historical event. A judgement based on other factors needs to be made, to arrive at a more balanced view.

To justify this conclusion, I shall list the places in the *BH Stuttgartensia* where there is textual evidence for emending Aram to Edom and *vice versa*, but then I shall seek to discuss other non-textual evidence and possible reasons for and against reading Aram or Edom in an example of each case. In other words, I shall be attempting to evaluate witnesses to a particular reading in a text rather than merely counting them. This list will be based on the occurrences of the words Edom and Aram in the *Dictionary of Classical*

⁹⁴ Clines, 1993, p. 17.

⁹⁵ Ulrich & Cross, 1995, ch. 54.

⁹⁶ Rahlfs' 6th edition, 1935, Württembergische Bibelanstalt, Stuttgart.

⁹⁷ The Peshitta Institute, Leiden: E.J. Brill, 1976.

⁹⁸ Ed. Paul de Lagarde, Göttingen, 1880.

⁹⁹ Revised by Robert Weber, Württembergische Bibelanstalt, Stuttgart, 1969.

Hebrew where the dictionary has recorded the emendations adopted in the dictionaries of Brown-Driver-Briggs and Koehler-Baumgartner as well as those mentioned in the critical apparatus to *BH Stuttgartensia*.¹⁰⁰

There are some scholars who think that there should be no place in a Hebrew dictionary for listing emendations. Frank Andersen in his review of volume 1 of *DCH* is much influenced by the lexicographical recommendations of James Barr. He writes:

The makers of *DCH* claim that ‘unlike previous dictionaries, *DCH* has a theoretical base in modern linguistics’ (p. 14). That claim can be studied by placing *DCH* alongside Barr’s recommendations.”... “Barr says that a dictionary ‘should not register text corrections simply because they have been proposed’.”¹⁰¹

The conflict between Andersen and Clines in the correspondence included in this review seems to be the result of the desire of Clines to make the *DCH* user friendly and to reach an audience wider than the academic community and of Andersen to be linguistically up-to-date and to be pedantically exact. Andersen approves the use of cognate words in a dictionary; Clines doesn’t. Each has their individual views, guided by the directions from which each is coming. I will go along with Clines.

The *DCH* in its policy makes it clear that it does not attempt to evaluate emendations or recommend particular emendations of the texts. Nor does it ever say that it agrees or disagrees with an emendation. What it does do is to provide for the reader of the dictionary a record that an emendation has been proposed, using in brackets after a reference, “*ms* or *mss* ****”; or, using in brackets after a reference, “*or em* [ended]”; or that such and such is the result of accepting an emendation, using in brackets after a reference, “*if em* [ended]”.¹⁰²

Yet, I must confess I am puzzled by the practice of *DCH*’s policy concerning emendations. Its Introduction says it does not evaluate emendations. It further states that “We always register a word in the places where it appears in the Masoretic text of the Hebrew Bible as it is printed in the edition of the *Biblia hebraica stuttgartensia*...”¹⁰³ But when I consult the dictionary under the word Edom (אֶדוֹם), I find listed as an occurrence Ezek. 27:16, but which in the *BHS* text reads Aram (אַרָם)!

Under Aram /Aramaeans there are nine examples where it is suggested that Aram/ Aramaeans could be replaced with Edom. They are as follows

¹⁰⁰ Cf the Introduction to the *DCH*, p. 17.

¹⁰¹ Andersen 1995, pp. 52, 69.

¹⁰² Clines 1993, p. 17.

¹⁰³ Clines 1993, p.17.

(note, without a Hebrew font, I have resorted to the text of the *RSV*. The key words which appear in the *BHS* are underlined):

1. EZEK. 16:57 BEFORE YOUR WICKEDNESS WAS UNCOVERED? NOW YOU HAVE BECOME LIKE HER AN OBJECT OF REPROACH FOR THE DAUGHTERS OF ARAM AND ALL HER NEIGHBORS, AND FOR THE DAUGHTERS OF THE PHILISTINES, THOSE ROUND ABOUT WHO DESPISE YOU.
The BHS margin reads: l c mlt Mss Edd Syriac *Edom*
2. II KGS. 16:6 AT THAT TIME REZIN KING OF ARAM RECOVERED ELATH FOR ARAM, AND DROVE THE MEN OF JUDAH FROM ELATH; AND THE ARAMAEANS [SIC] CAME TO ELATH, WHERE THEY DWELL TO THIS DAY.
The BHS margin reads: *[Rezin] prb add * l potius *Edom* * K mlt Mss Syriac Targum mss Vulgate mss w'rammim, Q Greek, Targum, Vulgate w'dmim
3. NUM. 23:7 AND BALAAM TOOK UP HIS DISCOURSE, AND SAID, "FROM ARAM BALAK HAS BROUGHT ME, THE KING OF MOAB FROM THE EASTERN MOUNTAINS: 'COME, CURSE JACOB FOR ME, AND COME, DENOUNCE ISRAEL!'"
The BHS margin reads: Samaritan Pentateuch m'arm; prp *Edom*; l M.
4. II CHR. 20:2 SOME MEN CAME AND TOLD JEHOSEPHAT, "A GREAT MULTITUDE IS COMING AGAINST YOU FROM ARAM, FROM BEYOND THE SEA; AND, BEHOLD, THEY ARE IN HAZAZON-TAMAR" (THAT IS, EN-GEDI).
The BHS margin reads: l c Ms Old Latin from *Edom*
5. II SAM. 8:12 FROM ARAM, MOAB, THE AMMONITES, THE PHILISTINES, AMALEK, AND FROM THE SPOIL OF HADADEZER THE SON OF REHOB, KING OF ZOBAB.
The BHS margin reads: nonn Mss from *Edom* cf Greek Syriac
6. I KGS. 11:25 HE WAS AN ADVERSARY OF ISRAEL ALL THE DAYS OF SOLOMON, DOING MISCHIEF AS HADAD DID; AND HE ABHORRED ISRAEL, AND REIGNED OVER ARAM.
The BHS margin reads: l c 2 Mss Greek Syriac *Edom*
7. EZEK. 27:16 ARAM TRAFFICKED WITH YOU FOR YOUR ABUNDANT GOODS, BECAUSE OF YOUR GREAT WEALTH OF EVERY KIND;
The BHS margin reads: l c mlt Mss Aquila Syriac *Edom* cf Greek *anthropous*
8. II SAM. 8:13 AND DAVID WON A NAME FOR HIMSELF. WHEN HE RETURNED, HE SLEW EIGHTEEN THOUSAND ARAMAEANS IN THE VALLEY OF SALT.
The BHS margin reads: pc Mss *Ed(o)m* cf Greek Syriac
9. II KGS. 24:2 AND THE LORD SENT AGAINST HIM BANDS OF THE CHALDEANS, AND BANDS OF THE ARAMAEANS, AND BANDS OF THE MOABITES, AND BANDS OF THE AMMONITES, AND SENT THEM AGAINST JUDAH TO DESTROY IT, ACCORDING TO THE WORD OF THE LORD WHICH HE SPOKE BY HIS SERVANTS THE PROPHETS.
The BHS margin has no suggested emendation, which means that *DCH*, with "or em. אֲרָם, *Edom*", must have derived this suggestion from either the Brown-Driver-Briggs or the Koehler-Baumgartner dictionary.

Under Edom/Edomites there are three examples where it is suggested that Edom/Edomites could be replaced with Aram. The key words which *should* have appeared in the *BHS* text are underlined. The problem here is that where *DCH* implies that the word Edom would be found in the text of

BHS, in fact the word *Aram* appears! There is some inconsistency here which I am unable to explain. The references noted as being susceptible to emendment are already covered under the lemma of *Aram*. The examples are as follows:

1. II KGS. 16:6 AT THAT TIME THE KING OF EDOM RECOVERED ELATH FOR EDOM, AND DROVE THE MEN OF JUDAH FROM ELATH; AND THE EDOMITES CAME TO ELATH, WHERE THEY DWELL TO THIS DAY.
2. II KGS. 24:2 AND THE LORD SENT AGAINST HIM BANDS OF THE CHALDEANS, AND BANDS OF THE EDOMITES, AND BANDS OF THE MOABITES, AND BANDS OF THE AMMONITES, AND SENT THEM AGAINST JUDAH TO DESTROY IT, ACCORDING TO THE WORD OF THE LORD WHICH HE SPOKE BY HIS SERVANTS THE PROPHETS.
3. NUM. 23:7 AND BALAAM TOOK UP HIS DISCOURSE, AND SAID, "FROM EDOM BALAK HAS BROUGHT ME, THE KING OF MOAB FROM THE EASTERN MOUNTAINS: 'COME, CURSE JACOB FOR ME, AND COME, DENOUNCE ISRAEL!'"

Having outlined the textual evidence, I would like now, as intimated above, to state other grounds for changes to show that the pressure for the emendations does not derive merely from the accumulation of textual witnesses, but also for other reasons. So, let us examine a few of these references and try to outline some of these other reasons and to consider some of these other grounds.

I have already adduced one example of the kind of non-textual arguments and reasons for changes to the biblical text in the case of II Kings 24:2, using, to some extent, geographical and historical data. But let me develop these further.

One scholar who believes *Aram* should stand in the text of II Kings 24:2, and goes further than most in defending his belief is D.J. Wiseman in his book *Nebuchadnezzar and Babylon* (1985). In discussing Nebuchadnezzar's operations against the west, for the years 599-598 BCE, Wiseman cites II Kings 24:2 as evidence for Nebuchadnezzar's support of raiding parties against Judah. Of this event he writes "The raiders are named as Arameans, Moabites and Ammonites by the Deuteronomist historian (II Kings 24:2). It has been supposed that the first should read 'bands of Edomites' on the basis of Jer. 35:11 and the Peshitta at both references".¹⁰⁴ In footnote 210, he cites Eph'al 1982, p. 172, n. 587, to this effect. He then give three reasons for retaining the reading of *Aram* in the text.

First, is a factor concerning historical geography, namely, that "the location of the starting-point of the current Babylonian operations in north and

¹⁰⁴ Wiseman 1985, p. 31.

central Syria at this time, as well as two years later, is against this".¹⁰⁵ Here he uses Bartlett's argument which is that, because Nebuchadnezzar's operations were conducted mainly from Syria, Syrian/Aramaean troops were more likely than Edomite ones to have been used against Judah.

But this ignores the Syrian/Aramaean reaction of hostility, resentment, fear and anger felt by the inhabitants of the Hatti-country¹⁰⁶ towards Babylon. According to the Babylonian Chronicle, for each of the previous six years (605/4-600/599 BCE) Nebuchadnezzar had led his army into this area, killed and taken prisoners, besieged cities, exacted heavy tribute such as precious metals and stones, livestock, hostages and idols and then, after that time, had used the thoroughly subdued land as a base for operations against the neighbours of the Hatti-country (the Arabs=Qedarites, Judaeans, Philistines). The Chronicle makes this abundantly clear.¹⁰⁷ So, it is practically certain, to judge from the need for the Babylonians' annual campaigns, that the area was difficult to control and that the Syrian/Aramaean people were bent on resisting the Babylonians, not cooperating with them. Rather than see Aram in the Babylonian organised *gedûdîm*, it could better be argued that just those tribal kingdoms which had not yet felt the weight of Babylonian oppression sided with the attackers in order to appease them or to escape the ravages of the Babylonian military. So, I conclude, we find Ammon, Moab and Edom aligned with Chaldaeans against Jehoiakim of Judah in II Kings 24:2.

Second, Wiseman draws a very long bow in support of his view that Aram should be read in II Kings 24:2. He uses possible historical reflections found in the apocrypha. He writes:

Also there is the tradition preserved in Judith (2:23-27) which tells how the Babylonian general 'plundered all the people of Rassis and the Ishmaelites who lived along the desert, south of the country of the Chelleans... He surrounded all the Midianites and burned their tents and plundered their sheepfolds. Then he went down into the plain of Damascus during the wheat harvest and burned all their fields and destroyed their flocks and herds, sacked their cities, ravaged their lands and put to death all their young men with the edge of the sword.' Greenfield has suggested that the details are based on the Babylonian Chronicle and that the Chelleans be identified with *chalon potamón* of Xenophon and the northern border of the Syrian desert along the al-Quweiq river outside Aleppo. This could

¹⁰⁵ Wiseman 1985, p. 31.

¹⁰⁶ When I refer to the Hatti-country, I have in mind such hinterland cities as (from north to south) Aleppo, Hamath, Tadmor, Riblah and Damascus. Coastal cities such as Arvad, Gebal/Byblos were more difficult for Nebuchadnezzar, with Babylon's great rival Egypt's triremes patrolling off-shore, supporting Tyre.

¹⁰⁷ Wiseman 1985, pp. 67-73.

explain the references to 'Arameans' since this was close to their traditional tribal lands.¹⁰⁸

This material, he readily acknowledges, is drawn from Eph'al 1982, pp. 173f.

But I do not find the connections he is trying to make very clearly stated. What are the two demonstrative pronouns, 'this', and the proper noun, 'Arameans', in his last sentence referring to? I do agree with Greenfield that the similarity in detail between the book of Judith and the Babylonian Chronicle, which relates Nebuchadnezzar's campaign against the Arabs in the neighbourhood of Carchemish, Aleppo and Tadmor, probably indicates that Judith's source for the historical geography was the Babylonian Chronicle. But what is Wiseman trying to show? His reference to 'Arameans' appears to be an allusion to the Babylonian general's raid on Damascus (a well-known Aramaean capital) during the wheat harvest, or it might be a reference to the Aramaeans in II Kings 24:2.

In this instance, does he want us to draw the inference that the events in the book of Judith illustrates the propinquity of Syrian Damascus to Judahite Jerusalem and therefore that the word 'Arameans' should stand in II Kings 24:2? If so, it is saying a lot to state the obvious. In any case, it does not follow from these historical reflections in the book of Judith that Aramaeans were likely to have been included in the Babylonian guerilla groups attacking Judah in ca. 603 BCE.

Third, Wiseman states in his book that "Strong evidence for the correctness of the 'Aram' tradition in II Kings 24:2 is the use of *Aramu*, with *Kaldu* and *Hatu* in a late Babylonian tribute list". Here he cites in support an earlier article he wrote.¹⁰⁹ But, I fail to see how the juxtaposition of Aramaeans with Chaldeans and Hittites (?) in one source, a tribute list, is "strong evidence" for 'Arameans' appearing in II Kings 24:2. The occurrence in the tribute list is purely fortuitous. The historical context needs to be known and considered in both cases.

In conclusion, I think Wiseman's contention about Aram being preferable to Edom in II Kings 24:2 is not supported by the evidence.

Let me at this point consider the text of Ezekiel 27:16, alongside II Kings 24:2, by way of supporting my contention about reading Edom instead of Aram. As reported above, the data on the text is as follows:

EZEK. 27:16 ARAM TRAFFICKED WITH YOU FOR YOUR ABUNDANT GOODS,
BECAUSE OF YOUR GREAT WEALTH OF EVERY KIND;

¹⁰⁸ Wiseman 1985, p. 31.

¹⁰⁹ Wiseman 1967, pp. 495-504.

The *BHS* margin reads: *l c mlt Mss Aquila Syriac Edom cf Greek anthropous*

Here, the textual evidence, unlike II Kings 24:2, clearly is in favour of reading Edom and not Aram. Nevertheless, there have been some scholars who, for historical reasons, have preferred to read Aram instead of Edom in Ezek. 27:16.

In 1968, M. Haran went to some lengths to argue that in Ezek. 27:16 (and also in Amos 1:6 and 9) Aram should be substituted for Edom.¹¹⁰ His discussion of Edom in Ezek. 27:16 is only part of his threefold argument that it was to Aram and not Edom that Tyre and Gaza “delivered up entire communities” (Amos 1:6 and 9).

Haran’s chief concern in his article was to defend the integrity of the book of Amos against scholars who tended to regard sections of chapters 1 and 2 as secondary, as late additions, and unlikely to have been written as early as the reign of Jeroboam II of Israel (ca. 786-746 BCE), as Amos 1:1 suggests. Haran attempted to construct an historical background for these early chapters which contradicted the claim that they contain late additions.

I will concentrate on his arguments as they relate to Ezek. 27:16 and I will, of course, argue that Edom should be retained. To begin, Haran states that:

Some scholars, on the basis of the Peshitta and Aquila versions which read Edom for Aram in Ezek. 27:16, want to substantiate the seeming testimony of Amos 1:9. However, it is more likely that in Ezek. 27:16 the proper reading is that of MT (corroborated by Symmachus and Theodotion), since among the commercial products enumerated there we find purple, embroidery and byssus (*buz*), which apply better to Aram than to Edom.¹¹¹

First, Haran attempted to weaken the textual evidence for reading Edom by juxtaposing Symmachus and Theodotion with Peshitta and Aquila, thus making it appear that there is an equal contest here. He omits the *l c mlt Mss (BHS)* or *l c ca 25 MSS (BH 7th edition, 1951)* which appears alongside the Peshitta and Aquila verses. The weight of these other witnesses favour reading Edom. Consequently, all modern English versions of the Hebrew Bible read Edom. Even the LXX which reads *anthropous* (‘men’ *’dm*) implies a misreading of *’dom*, Edom.

Secondly, the argument that the ‘commercial products’ better suit Aram than Edom is specious. The products consisted of jewellery (turquoise/emeralds/garnets, coral, rubies/carbuncles *NRSV*) and textile fabrics: embroidered,

¹¹⁰ Haran 1968, pp. 201-212.

¹¹¹ Haran 1968, p. 204.

dyed or plain (purple [cloth], fine linen and embroidered work- *NRSV*). There is no complete certainty about the meaning of most of the Hebrew words listed.¹¹² In any case, there is considerable evidence from Umm el-Biyara that Edomites engaged in weaving¹¹³ and, of course, there is the real possibility that some items might have been the result of transit trade.¹¹⁴

Other scholars who prefer to read Edom in Ezek. 27:16 also point out that Aram is already catered for in the list of traders with Tyre in v. 18, where Damascus covers for Aram. Haran counters this argument by pointing out the duplication of some places under different names throughout Ezek. 27. This point has some relevance and weight, but it hardly compensates for the evidence I have already adduced to support my contention.

Thus, I believe one can have confidence that here in Ezekiel 27:16, as in II Kings 24:2, there is good reason for reading Edom in place of Aram in the biblical text.

But, one reference where I think Aram should stand in the *MT* and ignore the suggested emendation, 'Edom', proposed by the *DCH* is Num. 23:7:

And Balaam took up his discourse, and said, "From *Aram* Balak has brought me, the king of Moab from the eastern mountains: 'Come, curse Jacob for me, and come, denounce Israel!'"

The BHS margin reads: Samaritan Pentateuch m'arm; prp Edom; I M.

It is patent, I think, that Aram should stand in the text. The context of the oracles of Balaam indicates that Aram and not Edom is in view. Chapter 22:5 clinches this, where it has Balak of Moab sending messengers to Balaam at "Pethor, which is on the Euphrates, in the land of Amaw" (or "his people"). Aram and the Euphrates clearly belong together.

To return to the key text of II Kings 24:2, the aim of discussing the problem of whether 'Aram' or 'Edom' should stand in the text is to strengthen my contention that Edom indeed played a part in Judah's downfall in 587 BCE.

The text we use (*BH Stuttgartensia*) is no doubt the best critical¹¹⁵ text we have available,¹¹⁶ but we need to exercise caution in accepting it, for textual

¹¹² Cf. for example, Cooke 1936, pp. 302f; Eichrodt 1970, pp. 379-381; Zimmerli 1983, pp. 47f; Greenberg 1997, pp. 555f.

¹¹³ Bennett 1966, 383f.

¹¹⁴ Lindsay 1976, p. 30.

¹¹⁵ I use the term 'critical' purposely, for while we do not always know the arguments behind the editors' decisions in *BHS*, the critical apparatus of *BHS*, in fact, often contains short evaluations of the important variants given in the *critical apparatus*. See, Tov 1992, pp. 373, 375. Cf. the comments on value judgements in text criticism in Scott 1987, p. 19.

¹¹⁶ Cf. the *DCH* pp. 30f., under *Sources*.

evidence is not neutral. It is not necessarily unbiased. The Masoretes who fixed and standardised the text of the Hebrew Bible had before them many manuscripts and a variety of readings. They were aware of variant readings, but so far were they from introducing alterations into the actual text of the Hebrew Bible that, even where the text was plainly wrong, they confined themselves to stating in the margin the reading which they held to be superior. This led to the practice of *Qere* ('read') and *Kethib* ('written'). Thus, they exercised their corporate judgment about what constituted the best, that is, the more accurate text. But what guided their judgment?

The choice and selection of texts, which they included in the Hebrew Bible, we assume was guided by their knowledge of their historical traditions, of the language and syntax of the texts, their view of the origins and value of the various manuscripts and their religious preferences. And these last mentioned were not always free from the influence of the politics of the day.

That being the case, when we are confronted by the possibilities or even probabilities of alternative readings or emendations in the *MT*, we need to remember the situation and disposition of the Masoretes when assessing these alternatives. If I prefer a certain reading of a text, I am basically in a very similar situation to them. Their knowledge and mine of the textual evidence are matters of degree and not necessary of kind. There is no superior objectivity at work in the case of the Masoretes. Anyone who makes a choice regarding a textual reading is subjectively involved in the process.

I propose, therefore, to use the *DCH* corpus of texts on Aram and Edom outlined above as evidence for my view of II Kings 24:2, namely, that the inclusion of the word 'Edomites' in it reflects the more likely historical situation.

But this use of, and appeal to, text critical matters to help decide the issue carries its own dangers. By an appeal to historical or geographic considerations, these considerations are themselves dependent/reliant on other texts in the Hebrew Bible which describe the historical incidents/events or geographic facts. Consequently, there is a tendency to circularity in the structure of my argument. An unavoidable situation, given the nature and limitations of the evidence.

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La Syrie-Phénicie des VII^e-IV^e siècles: Réflexions céramologiques à propos d'un ouvrage récent

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Abstract

The author maintains that G. Lehmann's thesis on Late Iron Age pottery in Syro-Phoenicia is a crucial step in advancing modern standards in this field of archaeology. The chronological framework of Lehmann's ceramic corpus is firmly based on, and integrated with, a fully documented and sound stratigraphy. In many ways, this work ought to be used as a basic handbook in future researches, owing to its methodological clarity in both analysis and synthesis, and comprehensive cross-references. Lehmann himself has offered a historical hypothesis in regard to "trends in the Local Pottery Development". For his own part, the author raises some problems of archaeological periodization and suggests also various perspectives on research into ceramics.

L'étude de la poterie ou céramologie est devenue, depuis le milieu du XX^e s., l'une des disciplines dont nous disposons pour intégrer la culture matérielle d'un site ou d'une région dans le champ de la recherche historique: elle est utilisée surtout lorsque qu'il s'agit de périodes et de lieux où manquent les références chronologiques d'une documentation écrite. L'abondance des objets disponibles fait qu'elle est susceptible de constituer des corpus régionaux d'une remarquable richesse qui contribuent à l'histoire économique et sociale des populations anciennes, et qui correspondent plus ou moins étroitement à l'histoire politique des régions concernées. Cependant, dans ce domaine, qui est à lui seul un vaste champ d'étude, il faut tenir compte de l'état des recherches dont l'avancement est

très inégal selon les régions, en quoi d'ailleurs il est directement lié à la pratique archéologique et à son propre état de développement.

1. La céramologie, discipline récente en archéologie syro-phénicienne

On sait que la discipline archéologique en Syrie et au Liban est restée longtemps marquée par une tradition "primaire" de dégagement des monuments visibles, pratique caractéristique de la seconde moitié du XIX^e s. à peine modifiée au cours de la première moitié du XX^e. Dès les années 1830/40, les États occidentaux confiaient des "fouilles" dans certains pays du Proche-Orient à des personnalités (souvent des consuls) qui opéraient avec l'aide de dessinateurs et d'architectes et dont les missions étaient placées en général sous la tutelle d'institutions d'épigraphie-philologie et d'histoire (histoire de l'art et histoire politique). Il s'agissait alors 1 — de rapporter une documentation descriptive et graphique (plus tard photographique) des monuments et des inscriptions gravées restant *in situ* et 2 — d'enrichir des collections privées et muséographique par prélèvement de sculptures plus ou moins monumentales, d'inscriptions libres ou démontables et de trésors d'objets considérés comme étant précieux et/ou artistiques (bijoux, monnaies, sceaux, etc). Peu importaient les méthodes et techniques d'approche des objets recherchés, puisque tout le reste (structures et mobilier communs), était rejeté pêle-mêle dans les déblais avec les matériaux terreux, tant qu'on n'a pas su reconnaître, lire et interpréter la stratigraphie¹. La céramique commune des sites ainsi "fouillés", et dont les tessons ont été définitivement éparpillés, est à jamais perdue pour la recherche.

Seuls des vases complets et quasi-intacts, notamment ceux qui appartenaient au mobilier des tombes, ont été publiés à partir du moment où l'on a pris conscience de la signification chronologique de leur typologie qui allait de pair avec celle concernant la stratigraphie des tells — comme l'avait déjà remarqué F. Petrie dès la fin du XIX^e s. La fouille du cimetière de Neirab fut, en Syrie, l'un des premiers témoignages de la nouvelle attention à cette classe de matériel².

C'est donc une archéologie balbutiante et quasi inconsciente des dégâts qu'elle faisait, qui a fonctionné dans le cadre du mandat français sur ces

¹ Dans l'entre-deux guerres de la première moitié du XX^e s., le Comte R. du Mesnil du Buisson s'improvisait archéologue et publiait un manuel intitulé *Technique des fouilles archéologiques*, Paris 1934, après avoir opéré de manière très grossière à Tell Mishrifé (Qatna) (1924-1929) et même provoqué des dégradations irréparables par des galeries souterraines à Khan esh-Sheikhun (1930)!

² Cf. A Barrois et B. Carrière, 1927, pp. 126-142 et 201-212.

pays, et même jusqu'aux années 1960. Il faut dire que l'archéologie française elle-même y a produit des publications qui montrent, parfois jusqu'à la caricature, une forme de recherche encore tardivement marquée par cette pratique "primaire", laquelle aboutissait à détacher les objets et l'architecture elle-même de leur contexte stratigraphique réel³, signe d'un retard scientifique certain dans les problématiques envisagées et les méthodes suivies.

Cependant, dès les années 1930, des travaux comme ceux de la mission danoise sur le site de Hama ouvraient, en Syrie, une autre pratique, mieux adaptée, respectant toutes les structures construites jusqu'aux plus humbles avec leur mobilier trouvé *in situ*, et par conséquent avec la céramique, surtout lorsque les vases se présentaient complets ou quasi-complets⁴. Ce ne fut que dans les années 1960 que l'archéologie en Syrie et au Liban appliqua des méthodes résolument modernes permettant d'intégrer correctement, dans l'étude tridimensionnelle et globalisante de ses chantiers, l'énorme quantité de matériel céramique qui en sortait⁵. La céramologie introduisait désormais ses nouvelles exigences scientifiques, sans pouvoir, malheureusement, récupérer les informations (artefacts et contextes) qui avaient été négligées dans les anciens chantiers. Quelques chercheurs ont pourtant montré que les publications des chantiers malmenés, qui décourageaient leur utilisation scientifique, pouvaient malgré tout être exploités⁶.

³ Outre les déprédations de fouilleurs officiels, il nous suffit d'évoquer les travaux de terrassement géométrique effectués sur le site de Jbail (Gubla/Byblos) par l'architecte M. Dunand, avec l'accord du Service des Antiquités. C'est lui encore qui fut responsable, avec un homologue syrien, des fouilles d'Amrit (tell et sanctuaire Ma'abed) jusqu'en 1968; seul le sanctuaire a été publié (cf. M. Dunand et N. Saliby, 1985).

⁴ La première publication définitive, par E. Fugmann, 1958, rend compte de cette méthode qui rapprochait déjà avec une certaine précision les objets trouvés de leur contexte architectural et stratigraphique.

⁵ Pour les sites de l'âge du Fer en Syrie, on notera en particulier les missions suivantes: Tell Rifat (anglaise), Tell Abu Danna (belge), Tell Afis (italienne), Tell Sukas (danoise), Ras el-Bassit (française), Tell Sheikh Hamad (allemande). Concernant le Liban, on retiendra quelques missions significatives comme Kamid el-Loz (allemande) et Tell Arqa (française), des travaux de sauvetage et des sondages ponctuels à Tyr, Khaldé (libanaises), qui ont également permis de remettre l'archéologie protohistorique sur une voie scientifique.

⁶ On n'oubliera pas l'essai courageux et couronné d'un succès certain, presque total, de P. M. Bikai qui visait à restituer la stratigraphie réelle du site de Jbail/Byblos en renvoyant les "artefacts", précédemment soumis à un cadrage géométrique abstrait (voir *supra*, n. 3), aux parallèles mieux définis du même horizon régional. Bien que relatif, ce succès fut apparemment pour l'auteur le point de départ d'une recherche de céramologie phénicienne par ensembles ou complexes régionaux s'appuyant sur des données stratigraphiques précises (cf. P. M. Bikai, 1978a; 1978b, pp. 47-56). En Syrie intérieure, la même démarche fut pratiquée mais la réalisation s'inscrivait dans de bien meilleures conditions, notamment sur la base des fouilles de la mission italienne à Tell Mardikh et à Tell Afis, (cf. S. Mazzoni, 1990, 1992).

2. Un manuel indispensable pour la recherche

L'espoir de maîtriser un jour la céramique de la Syrie-Phénicie restait permis: "opérer un rétablissement" général devenait un objectif réalisable, au prix, certes, d'un effort soutenu et d'une méthodologie critique, avertie et conséquente⁷. C'est maintenant chose faite pour les quatre siècles du "Fer Récent"⁸ et de la période perse. La thèse de Gunnar Lehmann récemment parue: *Untersuchungen zur späten Eisenzeit in Syrien und Libanon. Stratigraphie und Keramikformen zwischen ca. 720 bis 300 v. Chr.*, AVO 5, Ugarit-Verlag, Münster 1996, constitue en effet un tournant décisif dans la recherche concernant ces deux périodes relativement distinctes mais qui ont en commun de jouer un rôle de charnière dans les processus historiques du Proche-Orient ancien⁹. Cet ouvrage s'impose désormais comme l'outil indispensable pour tout progrès en archéologie concernant la période traitée, d'autant plus que le corpus céramique qui en découle s'intègre avec une grande précision dans une révision critique de la stratigraphie de tous les sites concernés appartenant à cette vaste région, y compris quelques zones bordières. En rendre compte est non seulement inévitable mais hautement profitable à la communauté des chercheurs, à commencer par soi-même. En outre, l'exploit scientifique qu'il représente contribue d'emblée à la relance de la recherche historique et non seulement céramologique, lui insufflant en quelque sorte une nouvelle dynamique. L'auteur s'y emploie déjà par un article: "Trends in the Local Pottery Development of the Late Iron Age and Persian Period in Syria and Lebanon, ca. 700 to 300 B.C.", *BASOR* 311, 1998, pp. 7-37. Pour ma part, bien modeste, je proposerai ici quelques linéaments susceptibles d'accompagner cette relance en me maintenant surtout au niveau de la discipline céramologique.

La contribution de G. Lehmann serait donc double à première vue: 1) un corpus céramique, 2) une interprétation historique des tendances de son

⁷ Le sens figuré de la métaphore du "rétablissement" qui relève du domaine sportif/athlétique (cf. *Dictionnaire de la langue française* "Petit Robert", Paris 1988, *su.* "rétablissement") me paraît convenir dans le cas présent, sachant qu'il exprime l'effort nécessaire pour atteindre un état d'équilibre, c'est-à-dire un état qui permet de progresser de manière relativement satisfaisante.

⁸ "Fer Récent" est une appellation nouvelle qui trouve, certes, une ferme justification dans l'ouvrage de G. Lehmann, mais qu'il ne propose guère que dans le titre correspondant et dans celui de l'article qu'il vient de publier dans *Bulletin of the American Schools of Oriental Research* 311 (cf. *infra* dans le texte).

⁹ Dans une brève évocation placée à la fin du premier chapitre de la chronique "Archéologie", intitulé "Syrie et Phénicie septentrionale et centrale", d'un Bulletin d'information (cf. J. Briand et J. Sapin, *Transeuphratène* 17, 1999, p. 93, nous avons utilisé le terme "événement" à propos de cet ouvrage, signifiant par là qu'il nous paraît remarquable à plus d'un titre, comme l'article présent s'emploie à le montrer.

évolution. En réalité, elle m'apparaît triple, car dès le travail méthodique de constitution du corpus, l'auteur offre à ses collègues des "entrées" en nombre quasi infini comme autant de pistes de recherche. Il s'agit en effet d'une sorte d'édifice dont la construction est détaillée, étape par étape, avec un soin tellement minutieux qu'il lui assure non seulement une solidité globale, mais aussi et surtout une accessibilité remarquable à chacun de ses éléments d'information, par conséquent une grande transparence. Par ailleurs, un va-et-vient entre la thèse et l'article sur l'évolution de cette céramique, chronologiquement calée par rapport à l'histoire politique de la région, permet des raffinements multiples sur ce sujet au niveau des correspondances et des décalages que la comparaison fait déjà apparaître de manière évidente; j'en donnerai quelques exemples.

Le terme de recherches (*Untersuchungen*) dans le titre de l'ouvrage peut paraître trop modeste et on peut penser qu'il induit le risque de faire sous-évaluer les possibilités d'utilisation du nouvel outil qui est avant tout un bilan critique fait pour servir de base documentaire et, précisément, d'outil de recherche. Cependant, le terme garde foncièrement une parfaite adéquation avec le travail accompli par l'auteur, à condition de lui donner un sens plein. Ce qui nous est présenté ici couvre un vaste ensemble de démarches de recherche propres à la céramologie, depuis la définition des objectifs jusqu'à la dynamique qu'impulse l'approche historique d'une base considérable de données judicieusement évaluées, base à la fois solide et transparente, rappelons-le. En même temps, ces démarches multiples se limitent à un certain stade de la recherche, à savoir, dans le cas présent, le cadrage chronologique du corpus réalisé. Assurément fondamentale pour mettre à jour la discipline qui fut si longtemps handicapée par des négligences rédhibitoires, l'étape en question est aujourd'hui franchie grâce à G. Lehmann. Les résultats qu'il a obtenus peuvent être affinés, ce qui est l'avantage considérable de la transparence de la construction méthodique proposée ici, véritable modèle déontologique. Enfin, en raison même de cette transparence, la recherche me paraît pouvoir rebondir et progresser bien davantage qu'elle ne le pouvait auparavant.

3. Un manuel maniable et transparent

S'agissant, au sens propre, d'un "outil" de recherche, il convient de présenter brièvement sa forme matérielle et son organisation aux futurs utilisateurs. C'est un ouvrage de format 17×24 cm, relativement maniable malgré son épaisseur (770 pages). On y distingue trois grandes parties, de contenu

et de volume plutôt contrastés. La première et la troisième qui sont, à coup sûr, celles que le lecteur utilisera dès l'abord, constituent 45% du volume: ce sont, dans l'ordre naturel de curiosité,

– les 213 planches de formes de vases (*Keramikformen*) pourvues de leurs principales références bibliographiques (au moins une par variante de forme), de leurs datations (relatives) et, souvent, des vignettes cartographiques de leur répartition,

– et, pour rentrer avec l'auteur dans le vif du sujet, les 97 pages initiales où il expose 1) la méthode qui lui a permis de maîtriser une documentation très hétérogène, quant à sa fiabilité notamment et 2) les résultats atteints, classements emboîtés et datations. Dans cette partie méthodologique, il s'agissait pour lui de partir de l'analyse des groupes de trouvailles (*Fundgruppen*) et groupes de formes de céramique (*Formgruppen*), toujours étroitement associés à leurs lieux de trouvaille (*Fundstellen* = *loci*)¹⁰, pour aboutir au corpus des formes et à une stratigraphie générale de la Syrie-Phénicie en termes de chronologie absolue. On trouvera un résumé synthétique de cette méthode, présentée au fil de ses étapes successives par l'auteur lui-même dans l'article de *BASOR* cité ci-dessus, de sorte qu'il me paraît superflu de le répéter ici. J'indiquerai seulement que la charnière du processus méthodologique réside dans la mise en évidence de huit phases diachroniques caractérisées par huit ensembles de formes associées, donc synchrones (*Assemblagen*)¹¹ au moyen de la corrélation mécanique des groupes-clés de trouvailles puis de leurs formes communes; l'auteur a pris soin de tester ce classement par un traitement statistique de sériation des *Formgruppen* obtenus dans la plus large palette possible de *loci* aux contenus parallèles, qu'ils soient pauvres ou riches, et que leur datation soit vague ou précise. La solidité de cette méthode tient au fait qu'elle évite les pièges de l'argumentation circulaire, véritable plaie qui risque de se développer chaque fois qu'une documentation est trop imparfaitement soumise à l'épreuve d'une critique interne et d'une confrontation externe, la plus large possible.

¹⁰ Encore fallait-il soumettre à un examen critique les rapports des chantiers archéologiques présentant des trouvailles datables du Fer récent (*Späteisenzeit*), afin de déterminer, site par site, les lieux de ces trouvailles (*loci* plus ou moins précis selon les publications) et les groupes de matériel associés à ces lieux (*Fundgruppen*), céramique comprise. Ce qui nous vaut un important catalogue raisonné de 185 pages.

¹¹ Ici, les "assemblages" sont des ensembles reconstitués à l'échelle de la Syrie-Phénicie à partir de formes de céramique associées, donc des ensembles synchrones; elles n'ont qu'une signification chronologique. Cf. G. Lehmann, 1996, 21 (n. 1): "Assemblagen haben in der Literatur oft als funktionale Einheiten verstanden, z.B. als Inventar eines Haushalts (Rice, 1987, pp. 300-301). Im Zusammenhang der vorliegenden Untersuchung wird jedoch nur auf die chronologischen Folgerungen — und nicht auf mögliche funktionalen Zusammenhänge der Assemblagen — eingegangen".

Au centre de l'ouvrage (deuxième grande partie) se trouve un contenu que l'on n'a pas l'habitude de rencontrer à cet emplacement: les catalogues détaillés et raisonnés, au nombre de trois et représentant ici 55% du volume. Celui des *Fundgruppen*, déjà évoqués comme constituant la base de la méthode suivie par l'auteur¹², est une analyse critique de chaque chantier archéologique ayant contribué à la documentation recherchée (plus ou moins bien publiée, plus souvent moins que plus), dont les groupes de trouvailles (céramique et non-céramique) sont présentés avec leur contexte stratigraphique et dans un ordre logique selon leur place dans la chronologie relative (*Assemblagen*). On trouvera donc ici une vaste remise en ordre des rapports archéologiques (parfois du matériel non-publié et des observations notées dans des carnets de chantiers, documents tirés des réserves et archives de certains Musées), l'exploitation des études ultérieures concernant cette céramique et enfin des informations provenant d'échanges directs, tout cela jusqu'au début des années 1990.

En découle le deuxième catalogue (76 pages sur deux colonnes) consacré aux formes de céramique classées site par site et par groupes de trouvailles tels qu'ils émergent du catalogue précédent. Autant de fois qu'elles sont attestées dans les publications ou les autres sources d'information comme spécimens isolés ou en grappes, ces formes y sont désignées selon leur classement dans les planches de la troisième grande partie de l'ouvrage et pourvues de leur définition diachronique comme de leur référence bibliographique.

Le troisième catalogue (153 pages sur deux colonnes) est également consacré aux formes de céramique, classées ici dans leur ordre numérique correspondant à celui des planches. Il s'agit en fait d'un commentaire détaillé des planches, forme par forme et variante (de forme) par variante. Ayant distingué 459 formes de la céramique dite "locale" (sans compter leurs variantes), l'auteur indique

1) les *Assemblagen* dans lesquels elles sont attestées;
 et 2) la série des groupes de trouvailles dont elles sont l'un des éléments constitutifs, et la référence bibliographique de chacun des groupes concernés, avec les figures et/ou planches illustrant leurs formes de céramique spécifiques. Ce catalogue comporte également 59 formes de céramique grecque importée et imitée (sans compter les variantes de certaines d'entre elles) ainsi que 27 formes de céramique chypriote (plus cinq variantes de la forme 1) avec leurs types de décor lorsqu'il y a lieu. Tandis qu'elles sont traitées dans le catalogue-commentaire de la même manière que la céramique

¹² Cf. *supra* et n. 10.

locale, les planches correspondantes (six seulement) n'en présentent qu'une faible sélection, d'où l'importance du commentaire qui renvoie le lecteur non-initié aux études concernant ces formes et leurs décors.

Il est évident que les *Untersuchungen* de G. Lehmann constituent un prodigieux pas en avant de la céramologie du Fer Récent et de la période perse en Syrie-Phénicie, un pas d'autant plus exceptionnel qu'elle avait un énorme retard à combler. Méthode et résultats se trouvant désormais à la disposition des chercheurs et étudiants¹³, son ouvrage présente une telle somme d'information rendues accessibles par plusieurs "entrées" logiques et une telle transparence méthodologique qu'il prend d'emblée le statut de référence indispensable et de clé ouvrant de nombreuses pistes de recherche. Il joue le rôle d'une base de départ solide et prometteuse pour un développement significatif de la discipline.

Sous le rapport de l'impulsion que produit l'ouvrage, on notera encore que la céramologie devient dans le cas présent, et peut devenir par un traitement semblable de la céramique d'autres périodes, une pièce maîtresse du développement scientifique de l'ensemble des disciplines relevant du champ archéologique. En effet, la céramique joue ici le rôle de fossile directeur de la stratigraphie comparative pour la mise en ordre des résultats de l'archéologie de la Syrie-Phénicie dans son ensemble. On est d'autant plus autorisé à souligner la valeur méthodologique d'une telle mise en ordre, jusqu'au niveau de la chronologie absolue, que G. Lehmann y est parvenu en faisant du complexe céramo-stratigraphique un système combinatoire fonctionnant en parfaite autonomie par rapport à l'histoire textuelle-événementielle, laquelle est toujours susceptible de court-circuiter le raisonnement des chercheurs et de les enfermer dans de vaines argumentations circulaires fondées sur des indices isolés et, de ce fait, souvent mal interprétés.

4. Une nouvelle dynamique de recherche

Le rôle de la céramique comme fossile directeur pour la datation des niveaux archéologiques n'est, certes, pas une nouveauté. Cependant, son utilisation à l'échelle régionale — ayant abouti à des résultats solides et significatifs grâce à la méthode suivie par G. Lehrmann et à son immense travail documentaire et critique — place désormais la céramologie en posi-

¹³ L'ouvrage de G. Lehmann est le type même des manuels d'enseignement et de recherche, abordable par son coût modeste, maniable par sa forme et son organisation logique, hautement profitable par la transparence des analyses et par les facilités d'accès à une documentation abondante et aussi totale qu'il était possible de la rassembler au début des années 1990, promis enfin à une diffusion relativement large.

tion de discipline majeure dans le champ des problématiques des missions archéologiques lorsqu'elles définissent leurs programmes. Déjà, l'auteur tire de ses recherches céramologiques des implications historiques, en termes d'évolution et de tendances dans le domaine de l'histoire économique: systèmes politico-économiques, voire socio-économiques, ayant structuré l'espace, systèmes insuffisamment documentés par les textes la plupart du temps, ou seulement à partir de certains points de vue. Dans ce domaine notamment, et en étant traitée de cette manière, la céramologie devrait logiquement élargir la place qu'elle occupe parmi les disciplines de l'histoire¹⁴.

Dans son article essentiellement descriptif: "Trends in the Local Pottery Development ...", *BASOR* 311, l'auteur consacre le dernier chapitre aux "Conclusions et implications historiques", ouvrant ainsi la voie à l'interprétation synthétique propre à la discipline historique. Il y souligne le fait que la rupture dans les traditions céramiques¹⁵ et dans la répartition spatiale des formes¹⁶, qui s'est opérée de toute évidence entre 580 et 540 et qui suggère des changements profonds dans le système économique interrégional, n'est pratiquement pas attesté dans les sources textuelles dont nous disposons¹⁷. Il en est de même de l'influence économique grandissante des Grecs à partir de 650, clairement attestée dans la documentation archéologique, tandis qu'elle est à peine évoquée par ailleurs.

Le changement radical du répertoire céramique qui émerge avec l'"assemblage" 4, vers 650 précisément correspond, certes, à la part grandissante de

¹⁴ Il suffit de considérer, à titre de comparaison, telle ou telle publication archéologique depuis les années 1960 concernant l'antiquité protohistorique et classique des régions périméditerranéennes, par exemple, pour constater la place importante qu'occupe la céramique dans la documentation des historiens eux-mêmes et dans leurs analyses détaillées. Autres exemples comme autant de reflets d'une telle documentation et de telles études, les synthèses historiques comme celle de D. et Y. Roman, 1997. Cet ouvrage, traitant des formes et modalités des échanges commerciaux sur une centaine de pages, place, entre autres objets mais en première ligne, les amphores étrusques, massaliètes puis romaines, comme jalons des pénétrations successives de produits de la culture matérielle méditerranéenne à travers la Gaule. On est en droit de penser que l'étude des grands systèmes économiques est une voie qui est ouverte également à la céramologie du Proche-Orient, régions intérieures comprises, comme cela se fait pour l'histoire des pays de Méditerranée occidentale.

¹⁵ La figure 15 dans l'article "Trends ..." (*Bulletin of the American Schools of Oriental Research* 311, p. 29) présente les huit courbes de connexions entre chacun des huit "assemblages" céramiques et le nombre des formes-types qu'ils ont en commun avec chacun des sept autres "assemblages". Elle fait clairement apparaître que l'"assemblage" 5 est au cœur d'une rupture majeure au sein de tendances assez régulières, voire d'un renversement temporaire de l'une d'entre elle et d'un redressement pour une autre.

¹⁶ *Ibid.*, Les quatre vignettes cartographiques de la fig. 14 du même article mettent en évidence l'aspect spatial de la rupture sous la forme d'une réduction drastique de l'espace occupé par la céramique de l'"assemblage" 5.

¹⁷ Cf. *ibid.*, p. 31: "These developments are more evident in the material record than in the historical sources".

la céramique grecque importée (et imitée) et à une standardisation des formes (bien installée et encore croissante jusqu'à la fin de la période considérée). Il pourrait aussi avoir été l'un des signes annonciateurs de la fin de la subdivision spatiale traditionnelle des répertoires de l'Âge du Fer entre les régions côtières et celles de l'intérieur, qui s'est opérée vers 600-580, autre changement qui donne l'impression d'une unification de ces répertoires. A y regarder de près, il s'agissait en réalité, comme le souligne Lehmann, d'une pénétration des produits côtiers (céramique comprise) vers l'intérieur du pays, et cela sans contrepartie céramique dans l'autre sens. Les échanges économiques sous-jacents étaient donc d'une nature particulière: la Syrie intérieure entraînait apparemment en position de faiblesse dans un système économique méditerranéen qu'elle n'était pas en mesure de contrôler pour en tirer profit.

L'auteur tente une explication historique. La défaite des États araméens et la soumission des Cités-États phéniciennes vers 720 avait permis leur intégration progressive dans l'Empire néo-assyrien. Le rôle économique des populations se trouva redistribué dès qu'il apparut que l'interaction entre Grecs et Phéniciens qui dynamisait leurs échanges donnait à ces derniers les moyens d'intervenir non seulement dans le bassin méditerranéen mais aussi sur les marchés de l'intérieur où ils pouvaient imposer leurs conditions commerciales hégémoniques¹⁸. Cette dynamique d'enrichissement commercial des Cités-États phéniciennes qui continuaient à jouir d'une certaine forme d'autonomie, notamment dans le domaine économique, était assurément encouragée par leurs suzerains qui en tiraient profit, à commencer par l'imposition d'un tribut substantiel. Entre les deux pouvoirs, le politique et l'économique, les populations de la Syrie intérieure, paraissent avoir été vouées à la production de surplus agricoles (relativement limités vu les contraintes physiques réionales et autres pesant sur leur potentiel économique). La dépendance politique qui les affectait ne leur permettait pas d'équilibrer leur relative faiblesse économique. Au cours de l'Empire néo-babylonien, le système d'échange inégal lui-même (qui avait été contrôlé par le pouvoir assyrien mais ne l'était plus guère) faillit s'effondrer, ne se maintenant que dans le secteur occidental du plateau alépin, secteur de Tell Mardikh (Matah) compris.

En encadrant les populations locales, en Syrie intérieure notamment, par une administration fortement concernée, voire directement intéressée, l'Empire achéménide semble avoir provoqué une reprise de l'activité économique. Les secteurs économiques de production et de commercialisation

¹⁸ *Ibid.*, p. 32: "All the evidence points to a Phoenician hegemony in the economic affairs of Syria".

auraient été favorisés, au prix d'une certaine réorganisation: grands domaines agricoles (et pastoraux?), routes et marchés se multiplient, les échanges entre les régions côtières et intérieures s'intensifient. Cela pourrait expliquer le retour en force de la céramique méditerranéenne en Syrie du nord avec la prédominance de la production athénienne dans les catégories de la vaisselle fine, tandis que les amphores de transport grecques et phéniciennes largement attestées semblent avoir été réutilisées dans les zones agricoles de cette région. Il reste à préciser quelle classe sociale aura tiré profit de ce regain d'activité.

On remarquera la limite orientale des attestations de la céramique méditerranéenne pendant la période achéménide, limite qui s'établissait apparemment sur la vallée de l'Euphrate. Considérée dans son ensemble, la Jezireh était, en effet, quasiment "vide" depuis le derniers tiers du VII^e s.: elle ne paraît guère avoir bénéficié de la reprise économique. Tout au plus, quelques secteurs comme ceux des sources du Habur autour de Tell Halaf (et du Balih autour de Harran?) peuvent avoir maintenu une certaine activité, par ailleurs faiblement attestée et seulement d'une manière indirecte, pas des tombes essentiellement¹⁹.

5. Périodisation archéologique et nomenclature

Faut-il rappeler ce fait? L'archéologie, ayant pour objet d'analyser les cultures matérielles du passé attestées dans leurs ruines et dans leurs aires respectives, ne peut distinguer que par approximation les phases chronologiques qu'elles ont traversées²⁰. Par nature, en effet, cultures matérielles et

¹⁹ Le seul autre site jusqu'à présent connu qui puisse évoquer une quelconque occupation en Jezireh à l'époque perse est Tell Sheikh Hamad, situé dans la partie basse de la vallée du Habur (pourvue de terrasses quaternaires cultivables par irrigation) mais à quelque 70 km en retrait de la vallée de l'Euphrate. Il s'agit apparemment d'un site d'étape et de contrôle sur un axe de liaison entre les deux voies principales E-W encadrant la Jezireh — ce site bénéficie de sa position à proximité du contact entre la basse vallée du Habur, à vocation agricole sous conditions favorables et la vallée moyenne plus ou moins encastrée dans un environnement de collines relativement propices aux pacages d'hiver qui étaient encore à cette époque. Un petit lot d'inscriptions araméennes y fut découvert au début des fouilles de la mission allemande dans le niveau de surface du bâtiment F (cf. H. Kühne, 1984, pp. 166-178; 1986-1987, pp. 221-241 et 242-267). Datées entre 550 et 500 selon la paléographie, elles pourraient au mieux témoigner d'une réutilisation du site au tout début de la période achéménide, après un hiatus d'occupation qui suivit celle, bien plus conséquente, de la période néo-assyrienne.

²⁰ Cette approximation des phases chronologiques de l'archéologie se double d'un décalage par rapport à l'histoire politique reconstituée à partir des textes et des données épigraphiques, décalage tout à fait normal, par ailleurs, puisque les cultures matérielles et les événements historiques ne peuvent se recouper que de manière indirecte et, au mieux, par effet de réponse.

aires culturelles ont des limites floues. Au mieux, c'est par dizaines d'années que les archéologues peuvent dater les périodes et sous-périodes et, d'une manière générale, pour la fin de la protohistoire et pour l'antiquité classique, ce sont des tranches moyennes de 50 ans qui sont estimées être les plus significatives, donc utiles. Et la céramologie ne déroge pas à une telle règle pratique.

G. Lehmann a donc inscrit ses recherches dans cette pratique archéologique raisonnable. Peut-être même verra-t-on, dans la périodisation qu'il propose, une sorte de confirmation interne de ce qui après tout n'est qu'un principe abstrait et commode à suivre, plus ou moins fondé sur l'expérience des archéologues et autres chercheurs? A tort me semble-t-il. Certes, l'indice 50, comme valeur moyenne appliquée à huit "assemblages", paraît correspondre parfaitement (trop peut-être) au cadre chronologique de quatre siècles que l'auteur semble s'être fixé comme objet de ses recherches. En réalité, les limites extrêmes des phases initiales et finales aux datations flottantes (750 ou milieu du VIII^e s. d'une part²¹, 330 ou 300 d'autre part²²) peuvent aussi bien construire un cadre de quatre siècles et demi. Par ailleurs, la répartition interne des assemblages semble se faire de manière très irrégulière, privant de crédibilité, au moins çà et là, la notion même de moyenne. Des écarts importants par rapport à la "moyenne" théorique des 50 ans apparaissent, en effet, à propos des "assemblages" suivants: 30 ans pour l'Ass. 1; 20 ans pour l'Ass. 2; 100 ans pour l'Ass. 6; 80 ans pour l'Ass. 7; 30 ans pour l'Ass. 8 (à moins qu'on doive l'élargir à 60 ans pour descendre jusqu'aux environs de l'an 300 av. J.-C.). Le flou des datations-limites et les écarts de module diachronique justifient assurément les clauses de prudence de l'auteur et probablement les hésitations dont font preuve les titres respectifs de l'ouvrage et de l'article lorsqu'on les rapproche. Or ils traitent le même objet d'étude. Le fait que le titre de l'article arrondit les dates aux deux changements de siècles, les plus proches des limites proposées à l'intérieur du texte, montre bien que l'auteur souhaite maintenir des marges suffisamment larges pour ne pas gêner la recherche à poursuivre. Ceci n'empêche pas qu'il suggère, en revanche, de relativiser le modèle courant de périodisation pour mieux valoriser les assemblages repérés (et leurs contextes stratigraphiques) avec leurs datations que la typologie comparative, en toute autonomie méthodologique, a déjà fournies, et fournira encore au fur et à mesure du développement des fouilles et des publications scientifiques.

²¹ Cf. G. Lehman, 1996, p. 90.

²² *Ibid.*, p. 92.

Les changements de tendances qui se sont manifestées au long de leur évolution — jusqu'à prendre la forme d'une quasi-interruption et d'un renversement temporaire, effet apparent de changements-seuils cumulatifs et de compensation relative — se concentrent dans une période médiane aux limites floues mais englobant l'"assemblage" 5 (ca. 580-540) et une partie de l'"assemblage" 4 (ca. 650-580, en tout cas 600-580 qui marquerait la rupture la plus nette dans les traditions de l'Âge du Fer²³. Il est préférable, me semble-t-il, de placer vers 600 le passage des Ass. 4 à 5 plutôt que vers 580. Par exemple, les nouvelles importations grecques de l'Ass. 4, qui sont datables avec une relative précision, et dont les motifs de décor de goût oriental (G14, G17, G18) montrent qu'elles furent produites avant tout pour l'exportation vers la Syrie-Phénicie, ont dû y arriver dès le début de leur production, vers 600. Par ailleurs, dès 650, la variété croissante des formes de la céramique grecque importée (21/42) qui est l'une des caractéristiques de l'Ass. 4 paraît être l'un des facteurs ayant provoqué l'abandon de nombreuses formes traditionnelles de l'Âge du Fer. On peut estimer que nous sommes en présence d'une sous-période qui termine nombre des traditions de cet Âge, bien que certaines des plus récentes perdurent sous forme de variantes (*mortaria*, cruchettes et jarres de transport), sans ouvrir de manière convaincante la culture matérielle de la période perse qui se manifestera largement par un regain de plusieurs groupes de formes du Fer II C. Enfin, concernant la sous-période initiale au sein de laquelle l'interaction économique gréco-phénicienne a reçu un nouvel élan grâce à l'ouverture des marchés de la Syrie intérieure, la différenciation que l'auteur retient entre les Ass. 1 et 2, même si elle se justifie par certains changements, pourrait être relativisée²⁴.

²³ *Id.*, cf. *supra* (n. 15), p. 30.

²⁴ Je ne mets pas en cause la distinction relativement forte entre les répertoires côtier et intérieur qui caractérisent chacun des deux "assemblages" 1 et 2, bien que l'écart des formes spécifiques des deux répertoires de l'intérieur face aux deux répertoires côtiers diminue sensiblement du premier au deuxième "assemblage" (il passe en effet de 62 formes sur un total de 73 à 32 formes sur un total de 57). Il n'en demeure pas moins que les deux assemblages côtiers successifs, qui comportent chacun 82 formes et variantes de céramique, présentant parmi celles-ci le même nombre de formes spécifiques, 14, ce qui constitue un pourcentage relativement faible (17%), signe d'une évolution normale au sein d'une phase d'une cinquantaine d'années, donc d'une homogénéité certaine. Le seul changement notable est le doublement des formes communes entre les répertoires côtier (11) et intérieur (25) d'un assemblage à l'autre, signe d'une certaine pénétration du répertoire côtier en Syrie intérieure: les Phéniciens auraient mis à profit l'ouverture des nouveaux marchés que la défaite des États araméens avait provoquée. Lorsque l'on compare les répertoires de l'"assemblage" 3 à ceux des précédents, on s'aperçoit que la variété des formes du répertoire intérieur continue de s'amenuiser, tandis que le répertoire côtier se maintient au même niveau que précédemment (82-81). Les formes communes aux deux répertoires de l'"assemblage" 3 se maintiennent elles aussi au même niveau

Se pose alors la question de la nomenclature de la périodisation. Malgré les insuffisances du concept “Âge du Fer” et sa relative inadéquation, on adoptera, comme G. Lehmann, la désignation Fer IIC pour les “assemblages” 1 à 4, mais en limitant le dernier, de préférence, vers 600. Les “assemblages” 6 à 8 peuvent être désignés Fer III²⁵, bien que la culture matérielle qui caractérisera le début de la période hellénistique en Syrie-Phénicie commence à poindre dès l’“assemblage” 6. D’un autre point de vue et en raison du rôle historique des nouveaux Empires, on pourra désigner les deux périodes considérées comme “assyrienne” et “perse” respectivement, bien que cette dernière soit un peu débordante en amont et davantage à l’aval si on la pousse jusqu’à vers 300. Reste alors une courte phase intermédiaire, correspondant à l’“assemblage” 5, qui porterait bien le nom de “babylonienne” si on préfère écarter celui de “Fer intermédiaire”.

La période étudiée, quelles que soient ses limites précises dont la question reste en suspens, se présenterait donc ainsi: deux sous-périodes regroupant respectivement les “assemblages” 1-2, 3 et 4 d’une part (durée de l’ordre de 150 ans) et les “assemblages” 6 à 8 d’autre part (durée de l’ordre de 210/240 ans), séparées par une phase intermédiaire relativement brève (de l’ordre de 60 ans, “assemblage” 5).

Ce propos fait de questionnement et de suggestions, veut être avant tout l’hommage d’un utilisateur du nouveau Manuel, dans l’état présent d’une lecture et d’une utilisation qui ne font que commencer. Les retouches que je viens de proposer concernant la périodisation et la nomenclature sont, bien évidemment, des tentatives incomplètes d’hypothèses à tester, en vue d’améliorations ponctuelles ou plus larges. Elles soulignent avant tout le travail monumental et solide réalisé par G. Lehmann qui offre un tel appui à une recherche pratiquement inépuisable. Grâce à cette nouvelle base documentaire traitée selon une méthode scientifique, la céramologie comme discipline se trouve relancée dans le sens d’une totale intégration chronologique de la stratigraphie et des formes céramiques. La base documentaire est ainsi devenue une base chronologique solide, le repère nécessaire et inévitable pour tout développement. Modèle du genre, l’ouvrage en

que ceux de l’“assemblage” 2. Cette relative continuité ne doit pas, cependant, nous faire minimiser les différences de répertoire entre les “assemblages” 2 et 3, alors qu’elles sont moins nombreuses entre les “assemblages” 1 et 2.

²⁵ Concernant cette période souvent appelée “perse”, l’appellation Fer III présente l’avantage de souligner l’autonomie indispensable à la discipline archéologique et de maintenir une intégration minimale du cadre chrono-stratigraphique à l’intérieur du domaine syro-palestinien, ce qui n’exclut pas diverses synchronies et diachronies de cultures matérielles plus ou moins spécifiques au sein de cet espace. On notera qu’elle est couramment utilisée chez certains spécialistes d’archéologie dite “biblique” (cf. V. Fritz, 1985, pp. 74 et 170-176).

question provoquera à coup sûr la présence à ses côtés de bases comparables traitant la documentation des périodes encadrantes, Fer I et II d'une part et périodes hellénistique et romaine d'autre part. Mais quelles que soient les périodes et régions considérées, la chronologie la plus fiable n'épuise pas pour autant le champ des recherches céramologiques.

6. Un élargissement des perspectives

Il existe bien d'autres domaines dans lesquels la céramique enrichit les connaissances et contribue à l'avancement de l'histoire: n'est-ce pas également un document technique, un jalon économique, un marqueur culturel²⁶? Avec raison, G. Lehmann a traité la céramique qui a été diffusée dans des aires culturelles relativement vastes, à l'échelle de la grande région syro-phénicienne. Mais on sait par l'ethnologie que de nombreuses productions de potiers (sans parler des productions domestiques ou assimilées) n'ont pas de zones de distribution aussi vastes, n'atteignent pas des marchés aussi lointains et ne risquent pas d'être imitées et répétées à de telles distances. Il faut admettre qu'il pouvait en être ainsi autrefois. La "petite région" est, en effet, un espace privilégié pour certaines catégories de vases qui se répartissent à cette échelle là, dans des réseaux de marchés locaux. Elle définit une aire d'affinités culturelles qui sont le plus souvent liées au mode de vie des gens, lequel conjugue de manière variable des modes de production, d'échange et de consommation²⁷.

Selon les sites fouillés, la céramique est l'un des documents témoignant de leur fonctionnement local et/ou régional. En céramologie, on aura donc

²⁶ Cf., par exemple, le volume collectif qui sort nettement des corpus descriptifs à visée typologique et chronologique, *Terre cuite et société. La céramique, document technique, économique, culturel*, Actes de la XIV^e Rencontre internationale d'archéologie et d'histoire d'Antibes, 21-23 octobre 1993, Juan-les-Pins 1994.

²⁷ On rappellera quelques cas notoires dans l'archéologie syro-palestinienne où s'impose l'échelle de la petite région voire du secteur. Le petit royaume d'Édom conservait, notamment au temps de sa vassalité dans le cadre des Empires néo-assyrien et néo-babylonien, des traditions céramiques présentant des caractères tellement spécifiques de texture, de forme et de décor qu'elles nous suggèrent au moins la forte prédominance d'un marché intérieur. Autre exemple, la mise en évidence d'un "marché intra-régional de la céramique locale" dans la moitié ouest de la plaine de Yizréel à l'Âge du Fer. L'étude de marmites à marques de potiers (artisans isolés plutôt qu'ateliers importants), provenant de plusieurs sites de la région, accompagnée d'études de micro-provenance, a révélé le fonctionnement à deux niveaux de ce marché: "d'une part, de véritables monopoles de production-échange-utilisation à l'intérieur de chacune des unités géographiques d'occupation (ville entourée de villages), d'autre part des échanges réciproques, mais limités, entre ces différentes unités à l'échelle de la région" (citations de notre étude de méthodologie historique, J. Elayi et J. Sapin, 1991, pp. 130-131 et version anglaise, 1998, p. 111; cf. A. Ben-Tor et Y. Portugali, 1987, pp. 224-235).

intérêt à procéder par plusieurs niveaux d'échelle spatiale. En effet, l'examen des ressources propres à chaque site fait partie intégrante de l'étude des divers systèmes de production céramique, y compris le fonctionnement interne de telle ou telle chaîne opératoire en vue d'adapter la production à certaines clientèles et/ou à des systèmes de distribution particuliers. Je me permettrai de renvoyer, à ce propos, à un exemple frappant qu'il m'a été donné de rencontrer tout récemment: la céramique commune trouvée *in situ* dans l'un des chantiers des fouilles récentes de Beyrouth (Bey 010), concernant les niveaux Fer III/Perse (IXa et IXb) d'un quartier du port phénicien²⁸.

La céramique en question correspond de manière stricte et exclusive à une production locale dont la localisation relative des ateliers de potiers et des zones d'approvisionnement (gîtes de glaise et de dégraissant) a pu être déterminée grâce aux données géomorphologiques disponibles. Cette céramique aisément reconnaissable par sa texture n'a été signalée jusqu'à présent que dans le secteur de Beyrouth, de même que les formations dites "superficielles" d'où les matériaux pouvaient avoir été extraits. L'étude qui en a été faite appelle donc, nécessairement et comme corollaire, une enquête serrée sur la céramique des sites locaux et régionaux.

Un autre particularité de cette production locale réside dans les techniques d'atelier qui y furent mises en œuvre et qui manifestent des signes d'inventivité plutôt inattendues, ce que nous avons qualifié de "créations en chaînes dans certaines catégories de formes (de céramique)"²⁹. Deux groupes de cas nous ont servi d'exemples pour justifier une telle interprétation.

Dans le premier groupe, caractérisé par des bords dont les séries de formes sont nettement apparentées les unes aux autres, on trouve des catégories de vases plus ou moins ouverts ou fermés, des cratères, des marmites et même des tasses à deux anses: seules les formes et dimensions conservées permettent de classer avec assurance la grande majorité de ces spécimens (souvent très incomplets) dans leurs catégories fonctionnelles respectives, mais certains laissent subsister un doute, d'autant plus qu'aucun indice provenant de la chaîne opératoire (finitions et cuisson comprises), dans les limites techniques de nos observations, ne permet de les discriminer³⁰. A propos de ce groupe, on a conclu qu'il s'agissait de "l'utilisation inter-catégorielle de formules techniques" qui fut poussée jusqu'"au point de rendre floues les limites (habituelles) entre les fonctions attribuées à chaque catégorie"³¹.

²⁸ Cf. J. Sapin et H. Sayegh in J. Elayi et H. Sayegh, 1998, pp. 53-110.

²⁹ Cf. *ibid.*, p. 74.

³⁰ *Ibid.*, pp. 68-71 et 74-75.

³¹ *Ibid.*, p. 74.

Le deuxième exemple de créativité et de formes relativement rares est un groupe homogène appartenant à la même catégorie fonctionnelle et formelle, les plats creux (“*mortaria*”) à pied annulaire. Dans ce cas, ce n’est pas un changement de fonction qui aurait pu être visé à travers l’inventivité technique des potiers, mais c’est l’efficacité elle-même de telle ou telle solution technique visant à tester la mise au point de certaines formes de fond et de pied: renforcement du fond qui s’arrondit et moulures diverses du pied annulaire qui permettaient un meilleur calage des vases au sol³².

Chacun aura certainement, provenant de sa pratique archéologique, d’autres exemples de ce genre d’ouverture de la discipline céramologique à proposer. Le double exemple que je viens de présenter sommairement est de ceux là. C’est, en effet, une ouverture pluri-disciplinaire qui, parallèlement aux enquêtes ethnographiques, s’offre aux archéologues et céramologues pour le traitement de la céramique tirée des fouilles. Mais il est clair que cette ouverture scientifique ne peut fonctionner sans l’appui de solides bases chrono-stratigraphiques. Pour ceux qui travaillent sur la Syrie-Phénicie à l’Âge du Fer, le champ est désormais à portée de main, grâce à l’ouvrage de G. Lehmann, honneur de la discipline céramologique et, à travers elle, de l’ensemble des disciplines archéologiques et historiques.

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³² *Ibid.*, pp. 62–63.

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An Archaeological Survey of the Erzurum Province, 1999: The Region of Pasinler

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Abstract

Compared to certain regions of Anatolia, the archaeology of the northeastern provinces is still poorly understood. This paper presents the results of the first season of an extensive archaeological survey of the Erzurum region that was conducted under the umbrella of the broader project based at Sos Höyük. The aim of the survey is to document the changes in settlement patterns in this highland region and thus complement the cultural sequence that is emerging at Sos Höyük. Fifty-seven sites, ranging in date from the Chalcolithic to Ottoman periods, were recorded during the 1999 season, which focused on the Pasinler plain, at the headwaters of the Aras river.

Introduction¹

Ancient and medieval sites in the greater Erzurum Plain marked staging points along one of the most significant routes linking Asia with Europe. It

¹ I would like to thank the Anıtlar ve Müzeler Genel Müdürlüğü for the opportunity to conduct a survey of the Erzurum Plain and the government representative, Akif Tarlan, for his assistance in the field. The survey was funded by the Australian Research Council and conducted under the auspices of the Project Director, Antonio Sagona. The field team comprised the following members: Claudia Sagona [Archaeologist], Akif Tarlan [Government Representative], Murray Clayton [Surveyor] and students of archaeology — Justin Boschetti, Jana Boulet, Fahri Dikkaya, Sebastian Elston, Tom Komadina, Joanna Richmond. The author is responsible for the illustrations and photographs in this article.

seems reasonable to expect that an exchange of goods and technology, and the transmission of knowledge took place along this corridor, through the ebb and flow of trade and through the movement of people. While ancient societies are not silent on the economic and political significance of this region, only key towns and broad geographical zones are documented in, for example, the annals of the Assyrians kings Tiglath-Pileser I [c. 1112 BC] and Shalmaneser III [857 BC],² or those of king Menna [c. 810-786 BC] and his son Argishti I [c. 786-764 BC], of Urartu, who described the area as the pass lands by which the mountains could be negotiated.³

Over the past six years, excavations at Sos Höyük (1994-1999), 18 km east of Erzurum, carried out by the University of Melbourne, have presented a new body of data on human settlement, land use, and ancient environments.⁴ An almost unbroken, stratigraphic sequence from the late Chalcolithic to Medieval times has significantly broadened the scope of archaeological research in the region. Pottery from the site and an extensive range of radiocarbon dates⁵ has provided a secure basis upon which to conduct further survey work in the Erzurum Plain. Hence, the periodization of new sites can now move beyond speculation to firm associations, as the majority of pottery recovered from the surface is identifiable in the Sos Höyük sequence.

This paper presents the preliminary findings of the first stage of the survey, which was concentrated in the Pasinler Plain, east of Erzurum (Fig. 1). One of the principal objectives was to extend our knowledge of the human occupation of the Aras River valley and to comment on the role that Sos Höyük played in the ancient economy of the region, through the location of hitherto unknown archaeological sites. It was anticipated that given the fertility of the region and its economically advantageous position, that Sos Höyük would be only one of a number of sites in the Pasinler Plain. Formerly only a few other ancient settlements had been documented: those that had never fallen from public awareness, such as Hasan Kale (Pasinler Kalesi, ES24)⁶ or the ancient rock-hewn caves east of Nenehatun (ES33), and a handful of other random discoveries, at Uzunahmet (ES42), a hilltop

² Although Tiglath-Pileser III [735 BC] marched into the region the texts of this campaign are fragmentary; see Burney 1966, p. 58.

³ Burney and Lang 1971, pp. 136-137, 147, 194. Urartian texts mention the region of Diauehi, considered by some to incorporate the survey area, in reference to their western campaigns as it was this kingdom which thwarted the expansion of the Urartian empire.

⁴ See Sagona, Erkmen, Sagona and Thomas 1996, pp. 49-50, for a preliminary account of the palaeoecology at Sos Höyük.

⁵ Sagona 2000.

⁶ 'ES' is the prefix adopted for the sites found in the Erzurum Survey, 1999. Details of the sites and locations are given in the listing that follows.

site, or the mounds at Tepecik (ES30) and Bulamaç (ES25). However, the period, or periods, of occupation of most of these sites have never been established.

It was not until the 1940s that the eastern highlands were drawn into modern archaeological debate through the investigations by İ. K. Kökten, who carried out a survey of eastern Turkey that incorporated the Erzurum Plain.⁷ Excavations at three large mound sites, Karaz, Pulur and Güzelova, by eminent Turkish archaeologists, H. Z. Koşay and his colleagues,⁸ have underpinned the chronological and cultural framework of ancient northeast Anatolia, particularly for the Bronze Age. And in the 1980s, A. S. Güneri⁹ conducted survey work in the Erzurum plain; a summary of these finds appeared in 1987.

Geographically the Pasinler valley comprises the wide alluvial plain of the Aras River as it meanders its way east, eventually draining into the Caspian Sea. Flanking each side are imposing mountains, Palandöken to the south and to the north, the Kargapazarı mountains, which both protect and limit the scope for human occupation and land use. As the valley lies some 1700 to 3000 m. above sea level, climate is a serious factor, at least in the modern era, in the success or failure of local economies: winters are long and severe, and from spring to autumn, there is a sense of urgency in many of the local rural activities which revolve around preparation for the next winter.

Even though the plain is generally well-watered by the river, its tributaries and natural springs, the area is criss-crossed by a network of canals that supply the numerous farm plots, large and small, that carpet the valley floor. Varying local conditions can be dictated by micro-environmental factors. In the north-western reaches of the Pasinler Plain, the vegetation tends to be sparse, but in the south-east crops are noticeably lush. Varying soils types, on the one hand lithosols, often very stony, and on the other, deep rich brown soils, chernozems,¹⁰ as well as rainfall fluctuations and proximity to reliable water supplies are likely to have played a part in the differences.

Poorly drained, low-lying areas are subject to swamp, but even these marshlands are exploited for animal fodder. Last century, the Erzurum region was noted for its diverse bird life, which nested in the swamp lands,

⁷ Kökten 1944.

⁸ Koşay and Turfan 1959, pp. 349-413; Koşay and Vary 1964; Koşay and Vary 1967.

⁹ Güneri 1987.

¹⁰ Lithosols are arid with little soil development and chernozems are dark soils high in nutrients. I am grateful to Jennifer C. Newton for this information. Newton is currently undertaking an intensive geomorphological study of the Sos Höyük environment.

and the collection of eggs was a significant aspect of the local economy.¹¹ Currently wheat, potato and market garden crops (such as tomato, capicum, cucumber, lettuce) are the mainstay of agriculture. Sheep, goats, cattle and horses are also raised.

Natural vegetation consists of alpine grasses and low shrubs that cling to the not infrequent rocky knolls which outcrop as the land rises to the foothills, particularly along the northern reaches of the valley. The area is generally treeless save for the few stands of poplar and birch planted around modern settlements and along the road margins. Obsidian, abundant at a source north of Pasinler and as cobbles in the Aras River,¹² was an important natural resource which was exploited in the past by ancient communities, and to a lesser extent so were outcrops of basalt¹³ and white flint.¹⁴

Survey area and method

The first stage of the survey was carried out in the Pasinler Plain over eleven days, from the 10th to 20th July 1999. The area surveyed extended north and south of the Erzurum–Pasinler road, from Nenehatun and Toparlak, west of Pasinler, to Taşlıgüney and Tepecik, east of Pasinler [Fig. 1].

Transects within the survey zone were chosen at random in an attempt to locate and identify the signature locations likely to produce evidence of human occupation and land use. We took advantage of newly excavated trenches, dug in preparation for the laying of gas pipe lines, and walked eight kilometers east to west at the centre of the flood plain. As the trenches were cut by mechanical excavators to a depth of c. 2–2.5 m, they provided a rare opportunity to assess whether siltation was great enough to have completely obscured low-lying settlements, and this indeed was found to be the case.¹⁵ Although our primary focus was on the location of ancient sites, the presence of historic structures, such as churches, bridges, and cemeteries, was not overlooked in the interests of their long term preservation and to provide a comprehensive account of the changes in settlement patterns

¹¹ Burnaby 1877, vol. 2, p. 73; Lynch 1901, vol. 2, p. 209.

¹² Brennan 1996, p. 27–36.

¹³ Areas of basalt scree have been observed north-west of Sos Höyük on the northern banks of the Çöğender tributary, and at Toparlak (ES 41) discussed here. Both quarries have quantities of basalt wasters.

¹⁴ White flint outcrops occur south-west of Kevenklik and also on the mountain slopes to the north of that village; the latter was reported by I. Thomas during his palaeoecological survey of the region.

¹⁵ See ES56.

throughout history. Wherever the terrain permitted, the team walked along the transects three metres apart.

Sites were recorded, photographed and, in the case of substantial settlements, detailed plans were made. A sample of pottery and small finds, such as worked obsidian, was collected from the surface of all sites investigated.

Survey results

The Sites

Some 57 sites were recorded, which included those already known in the vicinity of Sos Höyük (ES47-52).¹⁶ Substantial multi-period settlements (mounds or *höyük*s), occupied in some cases as early as the Chalcolithic period, were located in the plain: notably those sites near Kevenklik (ES1), Taşlıkaynak (ES17), Kurbançayırı (ES20), Tepecik (ES30; Pl. 1), Büyük Tüy (ES35; Pl. 2) and Saksı (ES36).

Further observations indicate that the surveyed area was occupied most intensely during the Iron Age and in Medieval times. Extensive Iron Age burial grounds at Altınbaşak (ES29) and Karavelet (ES38) suggest that corresponding settlements were located in the southern section of the survey zone.

Conspicuous features on the landscape are castles or forts. Those at Pasinler (ES24), Demirdöven (ES27) and Uzunahmet (ES42) have been documented and reports of others in the highland between Baldızı and Kurbançayırı will be investigated in the future. Preliminary observations of the pottery from these strategic sites suggest that they were occupied in the Early Bronze Age, Iron Age, Medieval and recent times. Whether they were fortified during the earlier periods, and to what extent, remains a matter of conjecture.

Worthy of note are the remains of a substantial road which runs east-west through the valley comprised in part, of large basalt flagstones;¹⁷ sections can be seen near Övenler (ES11) and at Yiğittaşı (Sos Höyük, ES52).¹⁸ These paved areas would appear to be linked to the natural corridors formed by the flat and broad upper river terraces and may have been located in areas prone to flooding. It is hoped that this path will be traced more extensively in the future. Judging by the age of settlements which are

¹⁶ Sagona, Sagona and Özkorucuklu 1995, p. 215-217, fig. 3.

¹⁷ The flagstones are worn and flat, but they have not been cut by wagon wheels.

¹⁸ Sagona, Sagona and Özkorucuklu 1995, p. 197, fig. 3, Yiğittaşı ES52 is marked west of Area C.

found along this road (see ES8, 10, 16, 17 and 23), it could date as early as the late Iron Age/Hellenistic period.¹⁹

The Finds

Some Chalcolithic pottery was recovered from the surface of the very large mound at Büyük Tüy, west of Sos Höyük. This friable ware is riddled with voids and gritty inclusions (Fig. 2:1-2) and coil construction was visible in the section. Early Bronze Age pottery, so well-represented in East Anatolia,²⁰ was found in small quantities at a number of sites, but only one small mound site, Taşlıkaynak (ES18), was found to be exclusively of the Early Bronze period with the distinctive hand-made, black burnished wares.²¹ Pots with thickened bellies and loop handles (Fig. 2:3-4) at the rim are represented among the finds.²² The gritty late form of Early Bronze Age pottery, first recognized at Sos Höyük, has been located at other sites in the valley (Fig. 5:2).²³ A fragment from a large open vessel (Fig. 5:1) found at Sakı is worthy of note. Its decorative design does occur in Early Bronze assemblages elsewhere in eastern Turkey,²⁴ but the fabric is quite red and the slip is waxy, qualities which are, to date, peculiar to that site. The rail rim and open shape can be likened to bowl forms from Georgia.²⁵

The Late Bronze period at Sos Höyük has pottery that is notable for its coil production. The vessel walls are frequently undulated, even on the exterior surface, despite slipped and burnished finishes. This streaky dark ware has also been identified at Karavelet and Kayalık Tepe (ES38 and 42; Fig. 3:1-2).

Pottery from the Iron Age burial grounds is characterized by hand-built vessels, some by coil technique, with grey fabric (Figs 3:3, 3:5-4:2). Dark-slipped bowls and pots, burnished surfaces and incised decoration, especially simple herringbone patterns (Fig. 3:3, 6),²⁶ or hatched triangles, are

¹⁹ The extensive range of ware types dating from the Late Chalcolithic to the Medieval periods will be fully outlined in the forthcoming excavation reports of Sos Höyük.

²⁰ Sagona 1984.

²¹ Small and shallow mounds were found in the survey of the Bayburt Plain; Sagona 1989, 426.

²² Cf. Sagona, Sagona and Özkorucuklu 1995, p. 204, fig. 7:1; Sagona, Erkmen, Sagona and Thomas 1996, 44, fig. 10:1-2, pl. VIII:d; Sagona, Erkmen, Sagona, McNiven and Howells 1998, 52, fig. 7:7.

²³ Sagona 2000.

²⁴ Cf. designs from Pulur (Sakyol) in Sagona 1984, vol. III, fig. 122: 233, 235.

²⁵ Cf. Sagona 1984, vol. III, fig. 50:3 from Dangreulis Gora.

²⁶ Cf. Sagona, Sagona and Özkorucuklu 1995, p. 206, fig. 9:12.

common (Fig. 4:2; see also 3:4 for impressed design).²⁷ Local red-slipped wares (Fig. 3:5, 7, 4:1-2) of Iron Age date are represented, but refined Urartian, red palace ware is quite rare (Fig. 4:3, 5) which corresponds to a similar dearth at Sos Höyük. Unfortunately few sites have escaped modern damage, but these necropolis were targeted to such an extent, that it is unlikely that any graves have been left intact.

Hellenistic wares (Fig. 4:7), or local copies (Fig. 4:6), do occur at sites in the valley,²⁸ but to date only one possible Roman sherd has been found.²⁹ Burial grounds with graves defined by large stone circles (ES48), similar to Area B near Yiğittaş, have been discerned at a number of locations, but unless they have been disturbed with tell-tale artefacts left on the surface, such identifications remain tentative. Pithoi fragments (Fig. 5:3) from Yeni Çakmak (ES32) came from two or three robbed graves; the large jars are similar to those found scattered around Area B and a date in the Hellenistic period is likely.

Some Medieval sherds can be compared to those at Sos Höyük;³⁰ they are characterized by wheel production, thin walls and some have ribbed or grooved surfaces (Fig. 4:10). A rusticated appearance results from red paint, haphazardly applied to the surface of lids and jars (Fig. 4:10),³¹ however these wares fall within a restricted time span, around 1300 AD at Sos Höyük. Hence, a large portion of wheel-made, red pottery, coated with thin wash as well as glazed fragments (Fig. 4:8-9) from the surveyed sites are likely to post date the Sos material.

Fragments of hand-made plates and bowls (Fig. 4:12) with a distinctive surface were found at Bulamaç (ES25) and a few other sites in the valley.³² These have a pearly, lustrous slipped and burnished surface in an array of pink, yellow, and orange colours. I have not seen this kind of finish on any vessels from eastern Turkey or further east into Georgia. It does have some similarity to modern ceramics produced by the potters at Çanakkale in the west of the country and it seems likely that it is a product of the more recent historic period.

²⁷ Cf. Sagona, Sagona and Özkorucuklu 1995, p. 208, fig. 11:1, found in the fields to the south of Sos Höyük.

²⁸ Hellenistic wares are at ES1, 5, 8, 17, 20, 21, 25, 29, 30, 31, 36, and 53.

²⁹ The sherds of one vessel came from Pasinler (ES24). They are made from a highly refined, pale clay, a very crisp and hard-fired ware, coated with a thick red-brown slip that chips off in rounded spawls.

³⁰ Sos Höyük Medieval wares have been published in Sagona, Sagona and Özkorucuklu 1995, p. 200-208, figs 6-7, 11.

³¹ Cf. Sagona, Sagona and Özkorucuklu 1995, p. 203, fig. 6:4, from Sos Höyük; Sagona, Erkmen, Sagona and Thomas 1996, p. 39, fig. 5:3.

³² Demirdöven (ES26), Tepecik (ES30), Nenehatun (ES33), Uzunahmet (ES42), two pipe line sites Altınbaşak (ES54 and 55) held the pearly lustrous ware.

Gazetteer of sites

Abbreviations: cm — centimeters; Dm — diameter; E — east, eastern; ES1 — Erzurum Survey site 1; H — height; km. — kilometer; m — metres; N — north, northern; S — south, southern; W — west, western.

ES1. MOLLA TEPE, (Kevenklik Site 1): saddle mounds and lower settlement, 2.8 km NNW of Sos Höyük; W of the Sos-Kevenklik village road; c. 800 m SW of Kevenklik; situated in lower foothills, the site is well-watered by springs and run-off channels from the mountains immediately to the N; visible from Sos Höyük which lies to the S. *Period of occupation:* Iron Age, Post Achaemenid/Hellenistic, Medieval. See Fig. 4:4.

ES2. KEVENKLİK SITE 2: historic cemetery comprising carved gravestones facing west; c. 40 x 60 m, situated on a low, natural rise surrounded by fields; c. 1.1 km SW of Kevenklik; on the E side of the village track as it travels N-S from Molla Tepe; the cemetery is visible from the top of ES1, Molla Tepe. *Period of occupation:* Historic; difficult to date.

ES3. KEVENKLİK SITE 3: possible ancient cemetery marked by substantial stone grave circles and one linear alignment similar to those at Yiğittaşı Site 2 (Area B); c. 40 x 70 m; small sherd scatter, dips in the land and back-filled areas suggest past tomb robbing; c. 1.6 km. SW of Kevenklik; fallow rocky fields, water lies over the hills to the S; the area is in a sheltered basin surrounded by rocky outcrops and hills. *Period of occupation:* Iron Age.

ES4. KEVENKLİK SITE 4: sherd scatter in the fields indicating the remains of a modest settlement; c. 1.7 km SW of Kevenklik; c. 2.2 km NW of Sos Höyük; watered by springs and channels; the site is sheltered with knolls and hills to the immediate S. *Period of occupation:* Early Bronze Age, Iron Age.

ES5. ÇÖĞENDER SITE 1: low rise with sherd scatter indicating the site of a small settlement or farmstead; c. 210 x 65 m; c. 600 m W of Çöğender; c. 1.2 km NE of Sos Höyük; W of a dirt track linking Çöğender and Kevenklik; fields lie to the N and E, swamp to the S and W; water is plentiful with river tributary immediately W of the site. *Period of occupation:* Early Bronze Age, Iron Age, Post-Achaemenid/Hellenistic.

ES6. ÇÖĞENDER SITE 2: this site is on a spur sloping S from a high rocky knoll; an extensive obsidian scatter, very little pottery or apparent architectural remains suggest that the area was used seasonally, perhaps as a hunting camp; four disturbed stone circles on low mounds of earth, possible grave sites, can be found at the northern limits of the spur; c. 900 m NW of Çöğender; the site is immediately E of a dirt track linking Çöğender and Kevenklik; swamp and a river tributary lie to the W and N; fields are located on the other sides; Sos Höyük can be seen from the higher reaches of the slope. *Period of occupation:* Early Bronze Age, Iron Age, Medieval and antique wares.

ES7. ÇÖĞENDER SITE 3: at least two³³ small tumuli, one is plundered with bone and some pottery scattered around; the site is located on the N outskirts of Çöğen-

³³ Numerous small mounds lie to the east of Çöğender that may prove to be tumuli, but they are undisturbed.

der village, on a natural rise N of the Çöğender River;³⁴ swampy land bordering tributaries of the river lie to the W; water is plentiful. *Period of occupation*: only a few scraps of pottery of uncertain age were found; possibly Iron Age.

ES8. ÇÖĞENDER SITE 4: river terrace settlement; c. 1.6 km NE of Çöğender; c. 1.4 km WNW of Övenler; c. 140 x 140 m; on the N side of a dirt track branching E from the Baldızı road linking the village to the main Erzurum-Pasinler road; the land drops rapidly to the Çöğender River flood plain to the S. *Period of occupation*: Early Bronze Age, Iron Age, Post Achaemenid/Hellenistic.

ES9. PORSUK SITE 1: a small disturbed mound is likely to be a tumulus, a small scatter of pottery was found in the fields around the tumulus; c. Dm 20 m; located on the hill slopes in wheat fields c. 1.9 km S of Porsuk; fertile land watered by a tributary running through swampy land to the W. *Period of occupation*: Iron Age.

ES10. ÖVENLER SITE 1: possible ancient grave circles, some with signs of plundering; more recent Islamic burial grounds were located on top of a terrace; c. 40 x 33 m; c. 800 m NW of Övenler; c. 60 m W of the Porsuk road; a river tributary flows S through swamp land immediately W of the site. *Period of occupation*: Late Iron Age/Hellenistic; recent historic.

ES11. ÖVENLER SITE 2: about 20-30 m of road, distinctive for its very large and irregular basalt flagstones; a small low bridge once forded the tributary which flows S to the Çöğender River; the road continues through the swampy ground bordering the waterway, E toward the river terrace site [ES8]; c. 300 m WNW of Övenler.³⁵ *Period of occupation*: Possibly Iron Age/Hellenistic.

ES12. ÇÖĞENDER SITE 5: historic bridge with two arches and central, conical-topped buttress; built of bluestone with decorative red stone trim; the bridge spans the Çöğender River in the N sector of Çöğender village. *Period of occupation*: said to be of 15th to 16th century date.

ES13. BALDIZI SITE 1: a long and narrow village site, c. 140 x 40 m, flanking both sides of the dirt track that runs E-W from Baldızı; extensive architectural footings are still visible, though some rock clearance has taken place; located c. 250 m E of Baldızı and in a protected position, nestled against the S slopes of the mountain range; a covered spring is located on the E of the site. *Period of occupation*: Medieval.

ES14. BALDIZI SITE 2 — a small conical mound, c. 20 x 15 m, possibly a tumulus ringed at the edges by large and irregular boulders; located on a natural spur overlooking fields to the E; c. 400 m ESE of Baldızı and with views W across the Pasinler Plain; a new road from Övenler runs to the east of the site; water canals run at the foot of the spur on the W side and the land slopes down to swamp. *Period of occupation*: Iron Age.

ES15. BALDIZI SITE 3: foundations of a small rectangular building, c. 5 x 3.5 m; its commanding views over the nearby ruins of the settlement ES13 and of the Pasinler plain suggest that it was possibly a chapel; some 10-12 metres E of the tumulus; see location description for ES14. *Period of occupation*: Medieval.

ES16. ÖVENLER SITE 3: mound, c. 50 x 35 m and pottery scatter; c. 1.9 km NE of Övenler in fields bordering the road as it branches E toward Taşlıkaynak; to the

³⁴ The river is known locally simply as Dere [river] or Çökender.

³⁵ Farmers in the area reported further lengths of stone paved road closer to Çöğender. The period of use of the road remains speculative, but an Iron Age date is possible considering the age of sites in the vicinity.

south are the wide flood plains of the Çöğender River as it meanders toward Pasinler; the site is located on part of the stepped river terraces; undisturbed other than ploughing in the surrounding fields; commanding views of the Pasinler plain; the river is to the south and water canals are cut to the S and W of the site. *Period of occupation*: Late Chalcolithic, Early Bronze Age, Iron Age.

ES17. TAŞLIKAYNAK SITE 1: an extensive mound; numerous robber's pits have been dug in the top and N flank of the site; large stone grave circles are located in the W sector of the mound; the site is located at a bend in the Övenler-Taşlıkaynak road; like site ES16, this settlement was located on the upper river terraces of the Çöğender River; modern water canals virtually ring the site; there are extensive views E and W across the plain. *Period of occupation*: Iron Age, Post Achaemenid/Hellenistic, Medieval. See Fig. 4:9, 11.

ES18. TAŞLIKAYNAK SITE 2: small mound with some rocky mounds formed by land clearance, pits may indicate robbing; c. 45 x 60 m; located on the upper river terraces as for ES16-17 and ES19; c. 1.4 km S of Taşlıkaynak; the site is ringed by water canals and fields and a small stream lies to the east; E of the Övenler-Taşlıkaynak road. *Period of occupation*: Early Bronze Age.

ES19. TAŞLIKAYNAK SITE 3: ruined church, stripped of all of its dressed stone leaving only the mortar and rubble fill; the church stands in the village of Taşlıkaynak which has re-used the stone in the houses. *Period of occupation*: Christian.

ES20. KURBANÇAYIRI SITE 1: substantial mound on the S outskirts of Kurbançayırı; robber's pits have been dug into the top and sides; at the W side of the road to Pasinler; c. 5 km from Pasinler; the land falls quickly to the flood plain and a major river tributary which lies to the W; the modern cemetery is situated immediately N of the site. *Period of occupation*: Late Chalcolithic, Early Bronze Age, Iron Age, Post Achaemenid/Hellenistic, Medieval. See Figs 3:7, 4:7.

ES21. PORSUK SITE 2: settlement extends over two mounds (natural?) and includes the field between them; c. 200 x 150 m; immediately south of Porsuk village; the W mound is the site of the modern village; pits have been dug across the W mound and the E mound is under dung; the site is in the foothills of the N highlands and falls away to the distant flood plain of the Çöğender River some 600 m away; deep gullies lie to the E and S of the site. *Period of occupation*: Iron Age, Post-Achaemenid/Hellenistic.

ES22. TAŞLIKAYNAK SITE 4: small conical mound likely to be a tumulus; c. 45 x 50 m; c. 1.1 km SW of Taşlıkaynak; c. 400 m N of the large mound, ES17, and visible from the top of the latter; W of the Övenler-Taşlıkaynak road; the tumulus is protected on three sides by small natural hills, but has suffered some minor disturbance at the edges; a stream lies 20-30 m W of the site. *Period of occupation*: Iron Age, Medieval.

ES23. TAŞLIKAYNAK SITE 5: this settlement is located along the flat upper Çöğender River terrace; c. 170 x 9 m; c. 900 m S of Taşlıkaynak; immediately E of the Övenler-Taşlıkaynak road; there is a small stream to the E and modern water channels have been cut at the S foot of the site. *Period of occupation*: Iron Age, Medieval.

ES24. PASINLER (Hasankale): a large castle, c. 60 x 80 m, dominating the modern town of Pasinler; The castle sits atop a natural rocky spur, part of the range to the N of the site; restored walls were built on Iron Age foundations; the impression of large water pipes in the mortar and stone on the E side of the upper city walls,

- suggest that the citadel was supplied by a complex plumbing system. *Period of occupation*: Early Bronze Age, Iron Age, possible Roman, Medieval and antique wares.
- ES25. BULAMAÇ: a large mound situated in the valley floor of the Çögönder River with extensive lower settlement; wall lines are visible at the surface and complex stratigraphy is apparent in the disturbed areas; commanding views of the Pasinler Plain and it would appear to have been immediately S of the old Erzurum-Pasinler road recorded on early 19th century maps, currently the road is some 450 m to the N; c. 8 km W of Pasinler; the river flows to the S of the site and there is a meandering tributary to the N. *Period of occupation*: Early Bronze Age, Iron Age, Post Achaemenid/Hellenistic, Medieval and antique wares. See Figs 3:6, 4:6, 10, 12.
- ES26. DEMİRDÖVEN SITE 1: a ruined church, its compound, c. 41 x 41 m, and surrounding settlement; the church walls stand as high as the roof in parts; situated high on the E slopes of the Hasanbaba Mountain, overlooking the village of Demirdöven; c. 1.9 km SW of Demirdöven; on the W side of the Demirdöven-Pasinler road; a system of ceramic pipes brought water from the upper reaches of the mountain to springs on the S side of the settlement. *Period of occupation*: Iron Age, Medieval and antique wares. See Pl. 3.
- ES27. DEMİRDÖVEN SITE 2: a ruined fort, or castle, is situated just below the summit of the Hasanbaba Mountain; the fort is visible from the church, ES26, and appears as a rise, breaking the natural line of the mountain slopes; the strategic significance of this location cannot be underestimated with commanding views E along the Pasinler-Horasan corridor; c. 2.8 km SW of Demirdöven; spring and run-off water to the N of the sites is likely to have been exploited. *Period of occupation*: Early Bronze Age, Iron Age, Medieval and antique wares.
- ES28. ARDIÇLI SITE 1: historic, six-sided tomb in reasonable condition made from well-dressed stone with slate roof; an Arabic inscription is carved over the door; on the E outskirts of the Ardıçlı village; E of the Ardıçlı road; the tomb dominates the modern cemetery compound; a river tributary flows to the S of the site. *Period of occupation*: Historic.
- ES29. ALTINBAŞAK SITE 1: natural (?) hill with extensive, but thoroughly plundered, ancient burial ground; c. 70 x 45 m; immediately SE of Altınbaşak; S of the Altınbaşak-Tepecik road; a river tributary flows to the W of the site. *Period of occupation*: Iron Age, Post Achaemenid/Hellenistic, Medieval.
- ES30. TEPECİK SITE 1: an extensive site comprising a large stratified mound, c. 60 x 60 m in the centre and under the village of Tepecik, a lower settlement, stretching out to the modern cemetery hill to the S of the village, and beyond to the gas pipeline, still further S; the site would appear to be some 300 m long from N to S; located on the S river terrace; the river lies immediately to the N of the village; a dirt road runs N-S through the village. *Period of occupation*: late Chalcolithic, Early Bronze Age, Post Achaemenid/Hellenistic, Iron Age, Medieval and antique wares. See Figs 2:3-4, 5:2; Pl. 1.
- ES31. BEŞİK TEPE (Tepecik Site 2): mound with some signs of minor disturbance; c. 1 km E of Tepecik; on the N side of the road running E out of Tepecik; located on the S river terrace; good views E up the Pasinler plain; Pasinler lies some 9.2 km to the WNW; the river is located to the N. *Period of occupation*: Early Bronze Age, Iron Age, Post Achaemenid/Hellenistic, Medieval. See Fig. 3:4.
- ES32. YENİ ÇAKMAK SITE 1: burial grounds and conical mound likely to be a tumulus; pottery scattered across the general area suggests that a small settlement

may have been located on the crest of the hill; some plundering has taken place; located c. 600 m N of Yeni Çakmak and the Yeni Çakmak road; the site is concealed in the lower foothills in the S of the Pasinler Plain, only the tumulus can be seen from the lower, Alvar-Alibezirğan road; no apparent water supply, but the presence of a modern village suggests that a water source exists. *Period of occupation*: Iron Age, Medieval. See Fig. 5:3.

ES33. NENEHATUN SITE 1: eroded, rock-hewn caves, thought to have been tombs; cut into soft and friable scoria/tuff on the N side and overlooking the main Erzurum-Pasinler road; c. 4 km NE of Nenehatun; the site was probably more secluded in ancient times, but now overlooks the pass to the Pasinler Plain; a seasonal water source in a deep gully lies to the W of the site. *Period of occupation*: Late Chalcolithic, Iron Age, Medieval and antique wares.

ES34. NENEHATUN SITE 2: large mound; the site is sheltered, nestled between two spurs, but some disturbance has occurred in recent years from minor building works; c. 200 m E of a petrol station, on the N side of the main Erzurum-Pasinler road; c. 5.1 kms NE of Nenehatun; the area is well-watered with swamp at the foot (S) of the site and natural springs to the S of the main road. *Period of occupation*: Iron Age.

ES35. BÜYÜK TÜY SITE 1: a very large mound; robber's pits have exposed wall lines and other ancient debris; commanding views E to Pasinler; the location is sheltered, nestled into the slopes of the highlands to the immediate W; the village of Büyük Tüy is situated c. 100 m SE of the mound; spring water is abundant in the village. *Period of occupation*: Late Chalcolithic, Early Bronze Age, Iron Age. See Figs 2:1-2, 4:1; Pl. 2.

ES36. SAKSI SITE 1: large oblong mound on the NE outskirts of Saksi; a section of a cyclopean wall is exposed on its lower SW flank; wall lines are visible on top of the mound and there is evidence of some disturbance; the site is located on the terraced slopes of the N mountain range; the river flows to the S and generally, the area has plentiful vegetation with numerous modern canals around the site. *Period of occupation*: Early Bronze Age, Iron Age, Post Achaemenid/Hellenistic, Medieval.

ES37. SAKSI SITE 2: small oblong and narrow mound on top of a natural ridge; it lies c. 50 m E of the large mound in Saksi village; some disturbance and land clearance has resulted in long piles of field stones to the W of the site; the location is described in ES36. *Period of occupation*: Late Chalcolithic, Early Bronze Age, Iron Age. See Fig. 5:1.

ES38. KARAVELET SITE 1: a large, ancient burial ground, thoroughly plundered; the site is located on a natural rise, the first line of the S foothills, c. 450 m W of Karavelet and S of the Alvar-Alibezirğan road; the village and the site are separated by a deep gully with a stream flowing along its floor. *Period of occupation*: Late Chalcolithic, Early Bronze, Late Bronze, and Iron Ages. See Figs 3:1-3, 4:2.

ES39. KARAVELET SITE 2: small settlement or farmstead in fields on the slopes of the S foothills; c. 1.3 km W of Karavelet; c. 50 m S of the Alvar-Alibezirğan road; a track leads S to the site; a deep gully with a stream at its base lies to the W and still functioning ceramic pipes bring water to a spring at the side of the field. *Period of occupation*: Medieval.

ES40. TOPARLAK SITE 1: mound at the end of a spur from the highland known as Höyükler Hill on the S side of the old Korucuk-Erzurum road, mid-way between the turn-off for Güllü and for Toparlak; sheltered position, surrounded by

highlands; views to Saksi village directly opposite on the N side of the valley and to Pasinler; a seasonal stream lies to the E and a major river tributary to the W. *Period of occupation*: Medieval.

ES41. TOPARLAK SITE 2: possible basalt quarry with substantial scree on the slopes below the outcrop; high on the Höyükler Hill behind and SSW of the mound site, ES40; c. 300 m from the road; location details as for ES40. *Period of occupation*: uncertain.

ES42. KAYALIK TEPE [Uzunahmet Site 1]: a substantial fort or castle on top of a large volcanic outcrop with extensive lava flows at its lower reaches on the W side; the village of Uzunahmet lies to the E and S of the hill; extensive ring walls of ashlar blocks have been disturbed at the SW end, but can be traced under the top soil running E-W; commanding views E toward Pasinler; streams are located S and SE. *Period of occupation*: Early Bronze Age, Late Bronze Age, Iron Age, Medieval and antique wares. See Figs 3:5, 4:3, 5, 8.

ES43. UZUNAHMET SITE 2: an extensive abandoned settlement lies on the W side of the fort, ES42; the footings of buildings are laid out in regular grid fashion and well-paved cobbled roads are clearly visible; location details as for site ES42. *Period of occupation*: Historic.

ES44. KURBANÇAYIRI SITE 2: historic cemetery with five elaborate Ottoman headstones dated to the mid-1700s; the cemetery is on the upper the river terrace in the NE of the Pasinler Plain; the cemetery is located on the W of the Büyükdere road to Pasinler, on the outskirts, NW of Kurbançayırı; area is well-watered and covered in vegetation. *Period of occupation*: recent historic.

ES45. KURBANÇAYIRI SITE 3: small ruined church in a garden compound with an established grove of trees; on the upper the river terrace in the NE of the Pasinler Plain; c. 150 m E of the Büyükdere road to Pasinler; on the E outskirts Kurbançayırı village; the area is well-watered and covered in vegetation. *Period of occupation*: Historic.

ES46. KURBANÇAYIRI SITE 4: historic cemetery c. 50 m from the Kurbançayırı church, site ES45; graves are marked by large irregular stone boulders laid out at regular intervals in lines, very similar to the cemetery on the outskirts of Yiğittaşı, i.e. site ES51; details of location as for ES45. *Period of occupation*: Historic.

ES47. SOS HÖYÜK [Yiğittaşı Site 1, Area A]: substantial mound and lower settlement, multi-period site currently under excavation; located on the N sector of the Yiğittaşı village; the area is well-supplied with water through the Çögender River and numerous springs and modern canals in the village. *Period of occupation*: Late Chalcolithic to Medieval. *Bibliography*:³⁶ Koşay and Turfan 1959, map 1; Koşay and Vary 1964, map p. 7; Koşay and Vary 1967, pl. 1; Sagona, Sagona and Özkorucuklu 1995; Sagona, Erkmen, Sagona and Thomas 1996, 27-52; Sagona, Erkmen, Sagona and Howells 1997, 181-226; Sagona, Erkmen, Sagona, McNiven and Howells 1998, 31-64.

ES48. YİĞİTTAŞI SITE 2 [Area B]: extensive burial grounds comprising grave circles of large and irregular boulders with central capping stone: located on top of the high N bank of the Çögender River, in and around natural rocky outcrops; c. 900

³⁶ In 1987 Sos Höyük was the subject of archaeological investigations carried out by Atatürk University (Erzurum) and Erzurum Museum, but the results were not published.

m NE of Yiğittaşı; the area is arid and stony. *Period of occupation*: Hellenistic. *Bibliography*: Sagona, Sagona and Özkorucuklu 1995, 215-216, fig. 3.

ES49. YİĞİTTAŞI SITE 3 [Area C]: burial ground; burials have proved to be pit burials: the area is extensively cut by deep trenches used for storage of potatoes and robber's pits; c. 400 m SE of Sos Höyük; on the E edge of the village and S of the track leading to the E fields; water canals are cut at the S foot of the site and a spring has been tapped to the NW. *Period of occupation*: Iron Age.

ES50. YİĞİTTAŞI SITE 4 [Area D]: pit burials have been reported at various times eroding from the hill side on the E edge of the modern cemetery and large plundered grave circles are located S of the cemetery; c. 500 m W of Yiğittaşı and on the N side of the old Erzurum-Pasinler road which runs E-W through the village; recent wells have been dug at the foot of the cemetery and the area is well-watered by farm canals and a river tributary to the S. *Period of occupation*: Middle Bronze Age.

ES51. YİĞİTTAŞI SITE 5: historic cemetery comprising gravestones, irregular large blocks similar to ES46, placed in regular lines aligned E-W; c. 180 m E of Sos Höyük; on a hill to the E of the cemetery are foundations of a small rectangular building, possibly a chapel; the cemetery borders a cement water canal at the N. *Period of occupation*: Christian.

ES52. YİĞİTTAŞI SITE 6: remnant of a road comprising irregular large stone slabs and ford over water channel similar to ES11; c. 85 m E of Sos Höyük; c. 15 m W of ES49. *Period of occupation*: Iron Age/Hellenistic.

ES53. TEPECİK SITE 2: small settlement or farmstead disturbed by gas pipe trenches; c. 1 km W of Tepecik; in swampy ground close to stream. *Period of occupation*: Iron Age, Hellenistic.

ES54. ALTINBAŞAK SITE 2: small settlement or farmstead disturbed by gas pipe trenches; on the W bank of a stream; c. 1 km N of Altınbaşak. *Period of occupation*: Medieval and antique wares.

ES55. ALTINBAŞAK SITE 3: small settlement or farmstead disturbed by gas pipe trenches; c. 1300 m NW of Altınbaşak. *Period of occupation*: Medieval and antique wares.

ES56. ESKİ KÖY [Alvar Site 1]: extensive settlement disturbed by gas pipe trenches; architecture was said³⁷ to have been exposed at a depth of 2 m; c. 2.3 km SE of Bulamaç [ES25]. *Period of occupation*: Early Bronze Age, Iron Age, Medieval. See Fig. 2:5.

ES57. BÜYÜKDERE: an historic water mill still in use and in excellent condition with threshing floor outside and extensive canal system powering the grinding stones; an inscribed corner stone indicated at least 75 years of use; located on the road leaving Büyükdere.³⁸ *Period of occupation*: recent historic.

³⁷ Farmers working in the field indicated that it was the site of an old village and that remains of a church were close to the road leading N out of Alvar.

³⁸ A pithos burial, said to have been washed out of the hill side behind the mill, was investigated by staff at the Atatürk University. No report of the find is known.

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Figures

1. Map of the Survey Area.

2:1. Büyüktüy, ES35. Hole mouth pot; HM; open and friable red 2.5YR 6/7 clay with voids and medium, white gritty inclusions in the paste; matt, weak red 2.5YR 6/4 slipped exterior; Dm 24 cm. Chalcolithic.

2:2. Büyüktüy, ES35. Open bowl; HM; open and friable grey N 6/0 clay with voids and fine to medium, white gritty inclusions in the paste; thick and crazed, reddish yellow 5YR 7/6 slip on both surfaces; smoothed not burnished; Dm 26 cm. Chalcolithic.

2:3. Tepecik, ES30. Cooking pot with lip formed on the inner rim; HM; heat-damaged, friable and flaking, pink 7.5YR 7/4 clay; medium coarse texture with voids and fine to medium, white gritty inclusions in the paste; slipped both surfaces, the interior is matt and weak red 10R 5/4, the exterior is dark grey N4/0 and burnished; Dm 11. Early Bronze.

2:4. Tepecik, ES30. Pot with thickened belly; HM; medium-textured, dark grey N 4/0 clay with some voids and a moderate amount of mixed, medium gritty inclusions; slipped very pale brown 10YR 8/4; the interior is matt and smoothed, the exterior is burnished; Dm 20 cm. Early Bronze.

2:5. Pipeline site, ES56. Tall-necked pot; HM; heavy pale brown 10YR 6/4-7/4 clay darkened near the rim; there are voids and fine to medium, mixed gritty inclusions and some mica specks in the paste; smoothed interior and lightly burnished exterior, Dm 20 cm. Early Bronze.

3:1. Karavelet, ES38. Open bowl; HM by coil technique; medium to large, mixed gritty inclusions in the paste; slipped both sides; matt, light olive grey 5Y 6/2-7/2 interior and mottled light grey 5Y 7/1, streaky burnished exterior; Dm 28 cm. Late Bronze.

3:2. Karavelet, ES38. Large and thin-walled vessel with everted rim; HM; dark grey N4/0 clay; coarse-textured with voids and medium to large, mixed gritty inclusions in the paste; thickly slipped on both sides; the interior is crazed, burnished at the rim and matt on the lower wall; the exterior is streaky and burnished; Dm 29 cm. Late Bronze.

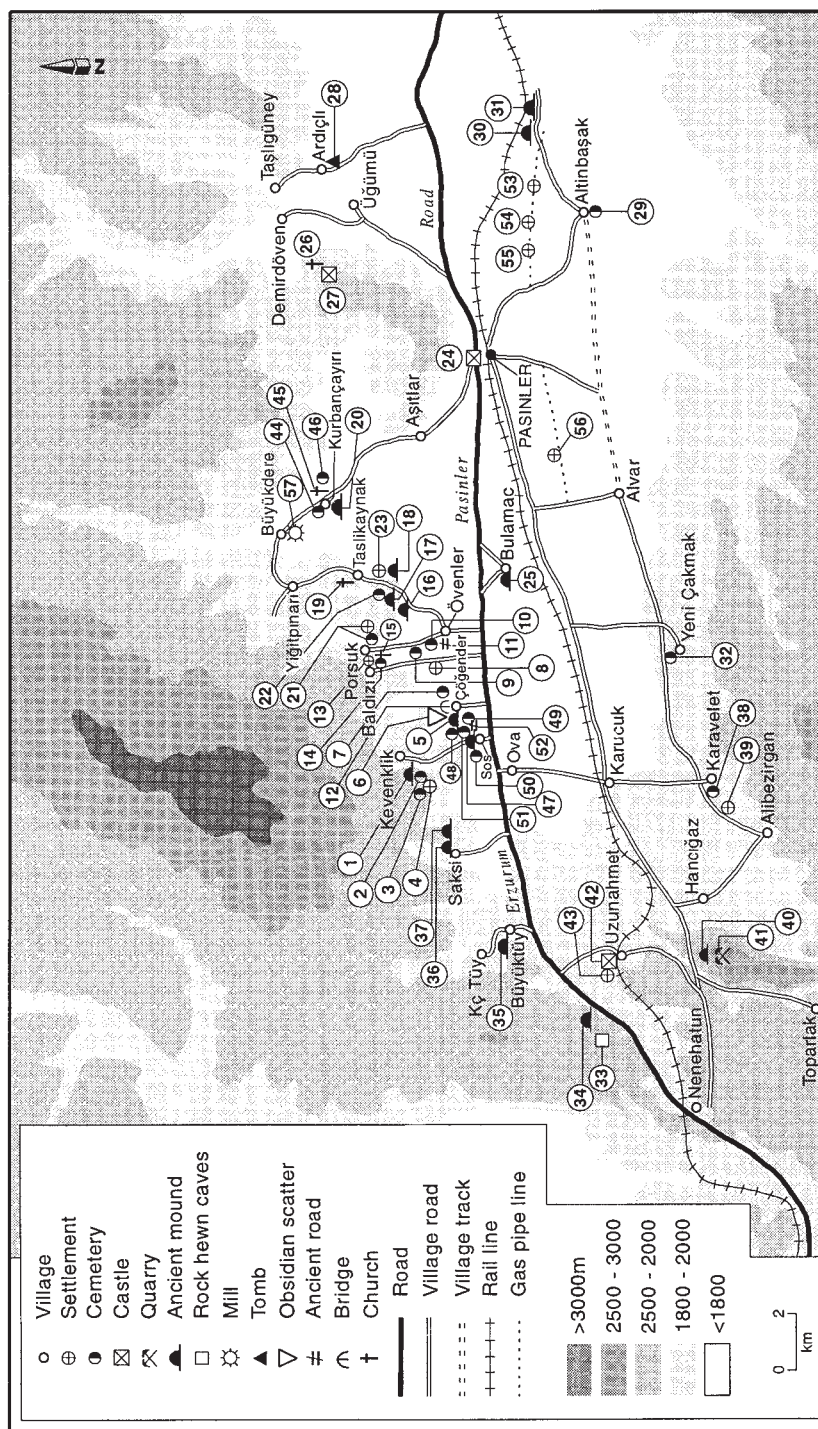
3:3. Karavelet, ES38. Carinated bowl with incised herringbone decoration at the off-set; HM; grey N5/0, compact and crisp fabric; mixed, fine gritty inclusions in the paste; slipped on both sides with a lightly micaceous clay; matt interior and burnished exterior; Dm 18 cm. Iron Age.

3:4. Beşik Tepe, ES31. Rim sherd with deeply impressed, crossed circle design on the top; HM; compact and dense-textured, pink 5YR 8/4 fabric with some fine to medium, gritty inclusions; thickly pink 7.5YR 8/4 slipped on both sides; the rim has been burnished; Dm 22 cm. Possibly Iron Age.

3:5. Uzunahmet, ES42. Bowl with off-set shoulder and flaring rim; HM; coarse-textured, dark grey N4/0 fabric with very large inclusions and voids, baking red 2.5YR 6/6 at the surface; slipped and burnished, light reddish brown 5YR 5/4-6/4 on both sides; Dm 24. Iron Age.

3:6. Bulamaç, ES25. Bowl with off-set profile and vertical rim; HM; compact grey N5/0 clay; pale yellow 2.5Y 8/2 surface; smoothed and lightly burnished interior and well-burnished exterior; incised herringbone decoration on the shoulder; Dm c. 16 cm. Iron Age.

- 3:7. Kurbançayırı, ES20. Closed pot with vertical, irregular rim; HM; grey 5YR 7/6 paste with voids and a moderate amount of mixed, gritty inclusions; thick and well-burnished red 10R 5/6 slipped surface; Dm 16 cm. Iron Age.
- 4:1. Büyük Tüy, ES35. Thin-walled pot with tab handle on the rim; HM; grey N5/0 clay; some voids and fine, mixed gritty inclusions in the paste; streaky burnished, light brown 7.5YR 6/4-7/4 exterior, darkened toward the base; burnished interior; Dm 15 cm. Iron Age.
- 4:2. Karavelet, ES38. Incised sherd; thick red 10R 5/6-6/6 and burnished slip on both sides; grey N5/0 clay, baking red 2.5YR 6/6; a small amount of fine to medium, gritty inclusions are in the paste. Iron Age.
- 4:3. Uzunahmet, ES42. Handle fragment with groove down the back; HM; light brown 7.5YR 6/4 clay with some fine, gritty inclusions; red 2.5YR 6/6 slipped surfaces; the interior is matt and the exterior is burnished. Urartian.
- 4:4. Kevenklik, ES1. Bowl; HM; grey compact, very dense-textured clay; a moderate amount of mixed fine, gritty inclusions is in the paste; light red 2.5YR 7/6 burnished on both surfaces; Dm 14 cm. Iron Age.
- 4:5. Uzunahmet, ES42. Body sherd with raised knob decoration; HM; grey 7.5YR 5/1 refined clay; thick red 10R 4/6-5/6 slipped and burnished surface. Urartian.
- 4:6. Bulamaç, ES25. Bowl with sharply off-set profile and lip on inner rim; a design has been rolled and impressed on the exterior; WM; chalky textured, light red 2.5YR 7/6 clay; highly refined paste with only a few minute grits; thin red 2.5YR 6/6, matt wash is on the surface; Dm 24 cm. Local form imitating Hellenistic ware.
- 4:7. Kurbançayırı, ES20. Rim from a thin-walled bowl; WM; very refined, light red 2.5YR 7/8 clay with no apparent inclusions; red 2.5YR 6/6 slip on both surfaces; Dm 10 cm. Hellenistic.
- 4:8. Uzunahmet, ES42. Rim sherd from an open vessel; WM; mid-blue glaze on interior and on exterior rim only; thin-walled; refined very pale brown 10YR 8/3 clay; Dm 5.5 cm. Islamic.
- 4:9. Taşlıkaynak, ES17. Rim sherd from a bowl; WM; dusky red 5R 3/3 glaze; refined light red 2.5YR 7/6 clay with no apparent inclusions; Dm 7 cm. Islamic.
- 4:10. Bulamaç, ES25. Large, thick-walled bowl fragment; the exterior is wheel-ribbed; grey clay with voids and fine, mixed gritty inclusions; first pale yellow 2.5Y 8/4 slipped, then pink 7.5YR 8/4 on the exterior, over which is a haphazardly applied red 10R 5/6 paint; Dm 28 cm. Medieval.
- 4:11. Taşlıkaynak, ES17. Plate fragment with wide rim; WM; abraded, smoke-blackened surface; medium-textured, light reddish brown 5YR 6/4 clay with a moderate amount of mixed, gritty inclusions in the paste; matt surface; Dm 12 cm, H 2 cm. Medieval.
- 4:12. Bulamaç, ES25. Base from a bowl; HM; grey clay, baking red 2.5YR 6/6; thin and micaceous, reddish yellow 7.5YR 8/6 wash has a striking lustrous pearl sheen; smoke-blackened on the bottom; deeply incised potter's mark at the centre of the ring; Dm of base 7 cm. Historic.
- 5:1. Sakı, ES37. Large storage jar fragment with rail rim; HM by coil technique; light reddish brown 5YR 6/3 throughout; matt, smoothed weak red 2.5YR 6/4 interior surface; burnished mottled exterior in red 2.5YR 6/6 tones; large voids from coil joins and fine to medium, mixed gritty inclusions in the paste; Dm 56 cm. Early Bronze.
- 5:2. Tepecik, ES30. Flaring rim from a large pot; HM; friable and sandy, reddish yellow 5YR 7/6 clay with a large amount of fine to medium, mixed gritty inclusions; the black exterior is slipped and vertically burnished, baking red at the rim; light red 2.5YR 7/6 slipped and burnished interior; Dm 36 cm. Early Bronze.
- 5:3. Yeni Çakmak, ES32. Rim from a large pithos; HM by coil technique; slightly darker clay at the core with some golden mica inclusions in the paste; slipped in similar reddish yellow 5YR 6/6 clay; Dm 40 cm. Late Iron Age/Hellenistic.



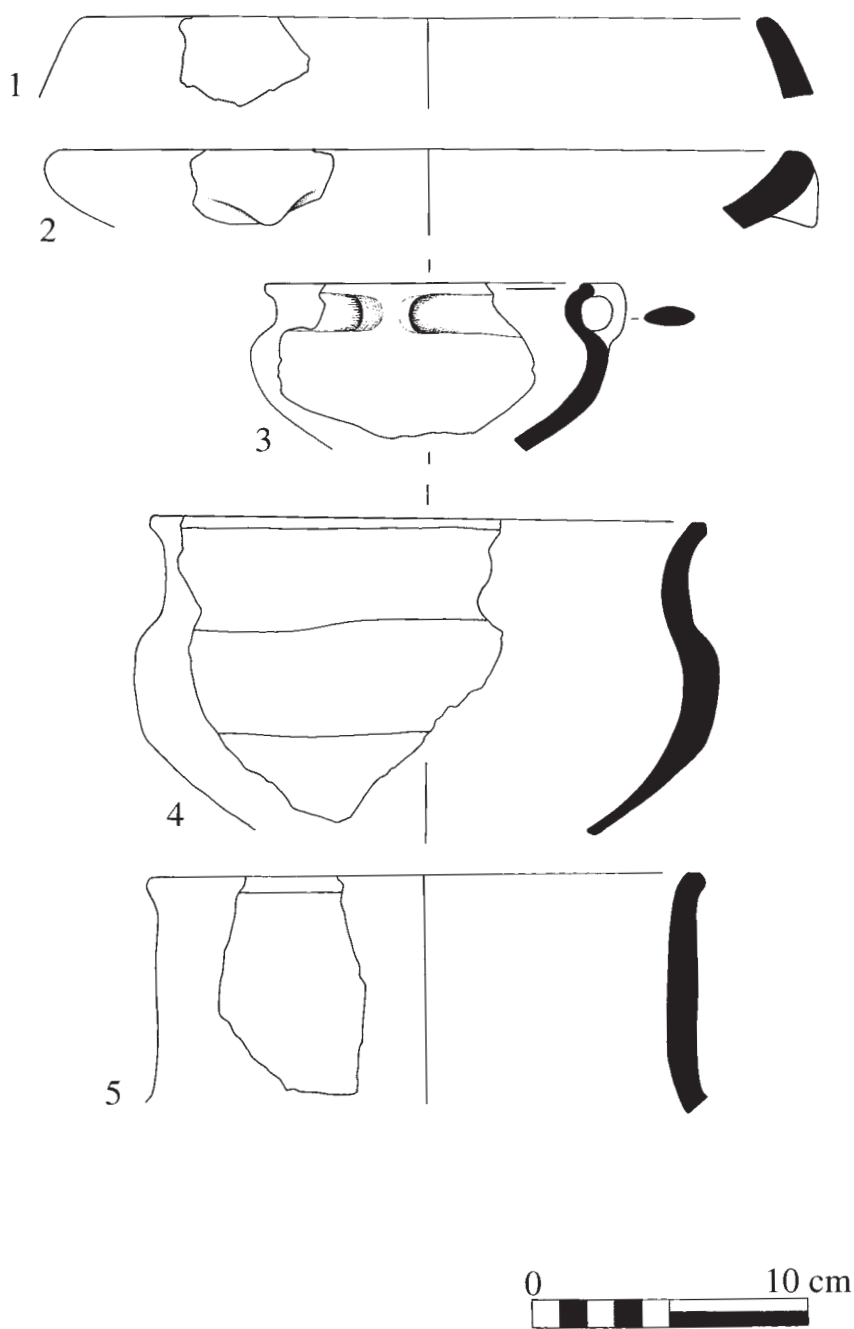


Figure 2

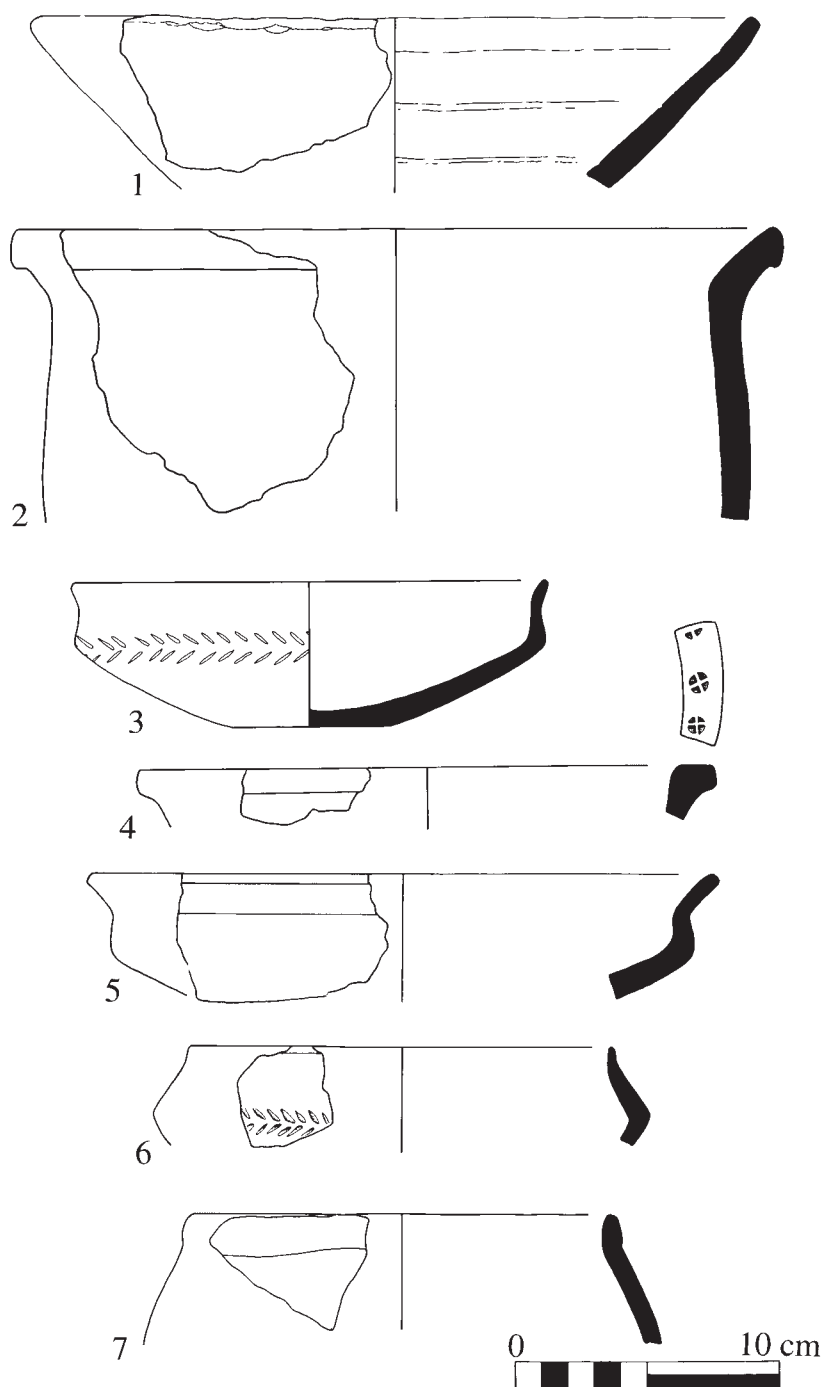


Figure 3

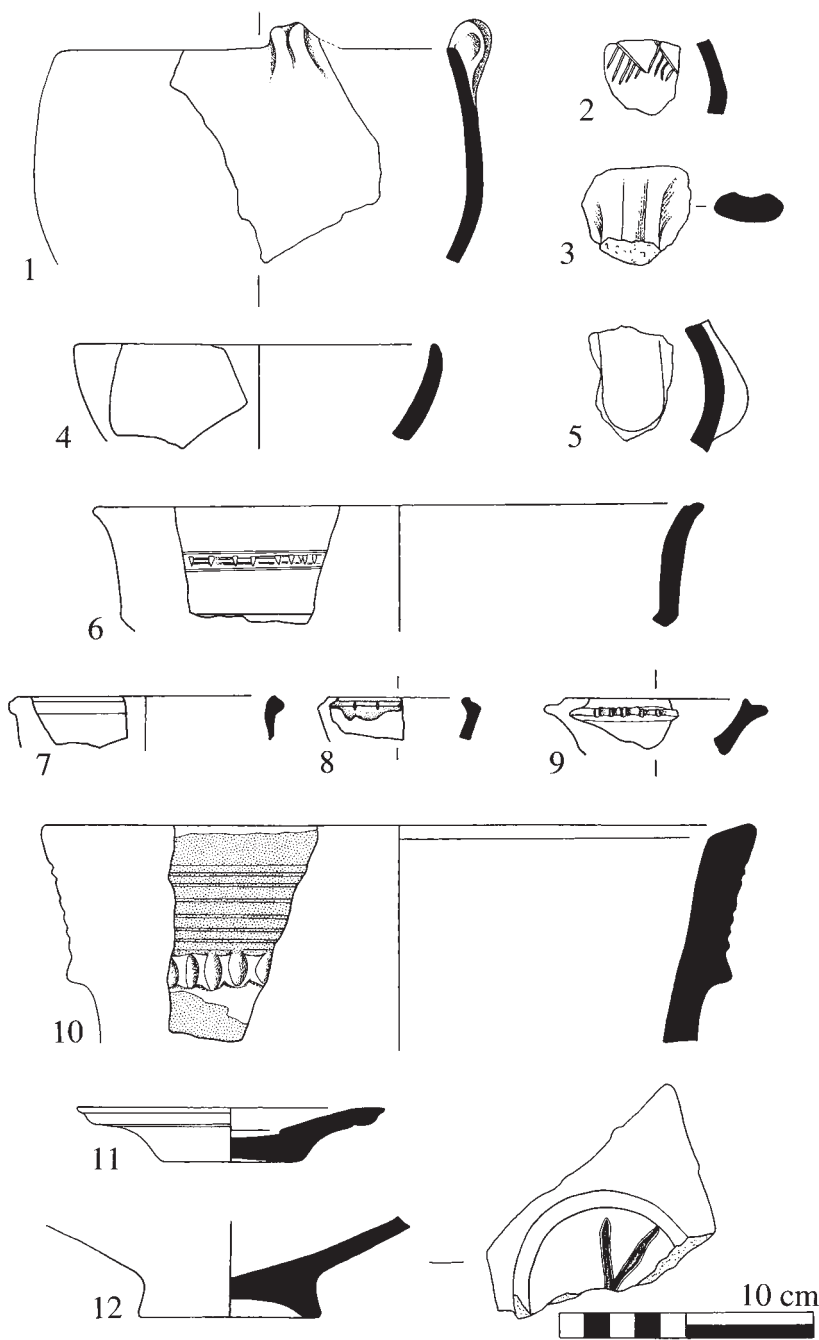


Figure 4

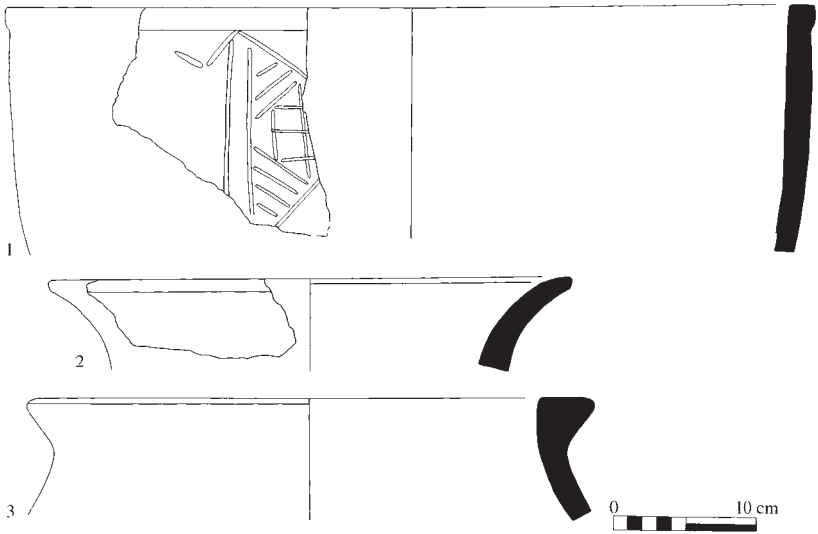


Figure 5



1. Tepecik Site 1 (ES30) mound, looking north-west.



2. Büyük Tüy Site 1 (ES35) mound, looking south-west.



3. Demirdöven Site 1 (ES26) ruined church, looking south-south-west.

Ethnoarchaeology at Sos Höyük: A Preliminary Report

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Abstract

Ethnoarchaeology is but one in a series of multi-disciplinary approaches required by contemporary large-scale archaeological projects. The role of the ethnoarchaeologist is to observe the correlations between contemporary behaviour and the patterning such behaviour creates in material culture. These observations may in turn be used to suggest behavioural correlates of ancient material culture patterning. The ethnoarchaeological research project at Sos Höyük has focused on observations of a functioning community occupying the same physical space as the ancient settlement, and noting the reflection, distortion and inversion of socio-ideological factors in contemporary material culture. Similar transforms may then be suggested to account for behavioural influences over ancient material cultural remains.

Introduction

Like most contemporary, large-scale archaeological research projects based in the Near East, the University of Melbourne project at Sos Höyük has assumed a multidisciplinary approach. It is no longer sufficient merely to dig up vast quantities of raw data and construct typologies of material culture. Sensitive, non-Eurocentric interpretation of information and reconstruction of living, working communities are required to carry archaeology into the future. A variety of specialised sub-disciplines has evolved within archaeology, that focus on understanding particular aspects of ancient lives: environment, trade, technology, and so on. The task of the ethnoarchaeologist is to broaden our understanding of these fields by observing their interrelationships with each other as they occur in a func-

tioning community, and relating these back to the material culture remains which constitute the core of archaeological evidence. Observing patterning in material culture and the transforms between behaviour and such patterned remains are the bases of ethnoarchaeological research.

Although much ethnoarchaeological research has focused on indigenous hunter-gatherer communities in the New World, some work has also been carried out with the long-settled village communities of the Near East. An analogy may be drawn between modern and ancient societies in places where broadly similar economic strategies have been carried out over the millennia within similar environmental constraints¹. It is important not to confuse the idea of ecological limitations with a concept of environmental determinism but rather to recognise the influence of physical environment on human behaviour ranges. No one, I think, would claim that 5000 years of subsistence agriculture and pastoralism was not accompanied by ongoing change in social, cultural, political and religious spheres. Tracing such changes through material culture is one of the biggest challenges for archaeology today.

The current ethnoarchaeological project at Sos Höyük has been designed to address specific questions of contemporary behaviour patterns in order to elucidate the processes of material culture production, use, discard and preservation. It is hoped that this will in turn allow an interpretation of the present day archaeological record in terms of past patterns of behaviour. The ethnographic survey of Yiğittaşı village focused on gathering modern data relating to aspects of demography, social organisation, cultural beliefs and economic strategies. Whilst the primary data should be of use in the very specific context of ancient Sos Höyük and its modern settlement, Yiğittaşı village, the results of the project must also be open to a wider application, in the form of a model of past to present transforms which may be used to test hypotheses at a range of other sites.

Ethnography

The Physical Village

The village of Yiğittaşı lies in the province of Erzurum in the far north-eastern corner of the modern Turkish nation-state. The village has a population of about 300 inhabitants, with few facilities, although being close to the main road there is a petrol station nearby. There are no shops, coffee-

¹ Orme, 1973, p. 483; Watson, 1979, p. 7

houses or other forms of private enterprise². It has, however, been designated by the Government as a central village, and is the location of several public services. The main service is a health clinic, staffed by a part time doctor and two full time nurses. There is also an office of the *kursal kalkınma* (or Village Improvement Service), and an associated residential block of four apartments although these buildings are no longer used for this purpose. Other public buildings in Yiğittaşı such as the mosque and primary school may be found in almost every Turkish village, however tiny or remote.

This series of public buildings in Yiğittaşı act as sites of inclusion of the village into the nation state. The mosque links the villagers to the wider Islamic world, whilst the primary school and clinic introduce the national curriculum and health service respectively. The *kursal kalkınma* office, now closed, integrated the village agricultural production into the national economy.

Architecture

There is a wide range of sizes and shapes of village houses, most notably in response to the topography of the site, and also perhaps for reasons of climate and economic necessity. The older, smaller dwellings which back onto the *höyük* are semi-subterranean, with stone walled rooms fronting excavated back rooms that are little more than caves. Another striking feature of village dwellings is the large number of private household residences under a single roofline. These dwellings share walls with one or more neighbours in a block-like building, although each family unit has a separate front door and front courtyard area, and does not have internal access to any other dwelling in the same block (Fig. 1).

The courtyard space in front of the door is important. Most of the older houses have one, although the houses dug into the mound do not, and some of the modern brick houses also lack such a space. Interestingly, one of the newer, freestanding houses recently had a concrete wall added to the front of it to create just such a yard. This space has a number of functions. It often links the front door with outbuildings or other rooms, such as a lavatory, storeroom or separate room in which the *tandır* oven is housed, and where daily bread baking is done. Chickens scratch around, washing is hung out to dry, water from the house is emptied and sheep are shorn here.

² The architectural remains of an abandoned, privately operated commercial dairy still stand in the southeast corner of the village.

The bare earth is frequently swept clean, and some small poplar trees may be planted.

All houses contain a corridor or entrance hall which forms the core of the home. This space corresponds quite closely to the kitchen of a Western style home, although the *tandır* oven may be located in a separate room off the corridor, or even in a separate building. Nonetheless, this is where food and cooking utensils are stored (Fig. 2), where most food is prepared and cooked over a gas burner, where tea is brewed and served, and where people gather to sit, chat, knit and smoke. It is the most informal part of the home and the area most frequently used by women (Fig. 3). Primary school aged children and older, retired men also frequent this space, whereas men of working age are more likely to spend time in a sitting or guestroom. In the older houses, which have stables under their roofs, animals and people may use the hallway for access. Thus it usually has an earthen or stone flagged floor which is continually swept clean. More modern houses, with separate barns and stables, have a concrete floor with linoleum or carpets.

The sitting/sleeping room or rooms usually open off this corridor. Often these are raised and access is via one or two steps. The concrete or timber floors are covered with rugs and shoes are removed at the steps. One wall is usually occupied by a built-in bench, covered with cushions for sitting on during the day. The family television is usually in this room. Bedding, including mattresses, sheets, pillows and blankets, is stored in wall units, or in niches actually built into the walls, and is brought out in the evenings, and folded away again during the day. Clothing may be hung on hooks on the walls, or stored in cupboards either in the room or in the corridor. These sleeping rooms are all approximately the same size, and are gender segregated. Fathers and sons sleep in one room, mothers and daughters in another. The only exception to this is when the youngest child is a boy who may continue to share his mother's bed until he is in primary school.

Thus almost all households require at least two rooms for sleeping. An all female household (a widow and her unmarried daughter(s) for example) could have only one sleeping room in use, however, whereas a household with a married son living in it would require a third room for the use of the married couple. Often this third room is available, being converted from a pre-existing guestroom. All the better off houses have such a room; a formal area for entertaining important visitors, sometimes separate from the rest of the house. Such a room is usually furnished with commercial furniture, is not used on an everyday basis and has a far more formal atmosphere than other sitting/sleeping rooms (Fig. 4). If a household has such a room it is often converted into temporary accommodation for a newly married

couple and, if the family is wealthy enough, is furnished with a commercially bought bed rather than the usual woollen mattresses. Such a bed may also be part of the furnishings brought with her by the new bride.

The presence of some abandoned and ruined houses in the village is a further reminder that not all buildings are necessarily occupied simultaneously. The process of decay of these traditional stone houses could be instructive in elucidating formation processes in the archaeological record (Fig. 5). What remains of abandoned houses is simply the stone external walls. All timber from the roofs, doors, doorways and windows is removed immediately for recycling in other constructions if possible, either immediately or at some later date. Internal walls don't seem to survive well and earthen floors are soon overgrown with weeds. Over time even the stones of the walls may be gradually removed to be reused for other purposes. This process is not random, however, as even the most decayed and overgrown buildings are still recognised as belonging to a particular individual or family, who retain the right to both the land and the building materials on it.

Population

The population of Yiğittaşı consists of 292 people living in 43 separate households. The smallest household has 3 members and the largest, the *muhtar's*, has twelve. The average number of individuals per household is six. Most households consist of a nuclear family, that is, a married couple and their unmarried offspring. These families make up 35 households or 83% of the village. In some cases one parent is deceased and the family is headed by a widow or widower. In three cases a single grandparent also lives with the family.

The rest of the village households (17%) consist of extended families, where one or more married sons live with their natal family. Generally the married sons move out and establish an independent household at the birth of their first child, as long as one or more siblings remain at home with their parents. In only one case was there more than one married son in the same household. In this case the household consisted of a married couple, their two eldest sons together with their wives, and three younger, unmarried children. The elder of the married sons has two sons and a daughter of his own, also living in this household, whilst another daughter has married and is living elsewhere. There are no other cases of extended families living together, although very often the establishment of new households occurs by building a separate room or rooms adjacent to the parental home.

The majority of village households are economically self-supporting through a combination of agriculture and animal husbandry, although outside wage labour is playing an increasing role in village economics. There are two village families which own no land and do not carry out agricultural production. They are the specialist sheep and cow herders, and meet their household grain requirements through payments by other villagers in return for pasturing the animals. The social status of these households is distinctly different from the bulk of the population.

Other villagers with distinct social roles are the *muhtar* and the guard. The *muhtar*, or village headman is elected by the villagers, and, as the lowest rung in the official government hierarchy, mediates between the village and the national administration³. The role of the *muhtar* is to levy taxes, initiate works projects around the village and adjudicate in village disputes⁴.

The other villager with a distinctive status is the village *bekçi* or guard. He is paid a certain amount of grain each year by each villager household, and is required to watch out for thieves, including the theft of crops, and to act as a town crier, notifying villagers of important information. In the 1990's, this information is often relayed via the loudspeaker at the mosque. An example of the type of information thus disseminated is the arrival in the village of the veterinary surgeons from Erzurum who were carrying out vaccinations of the village flocks and herds.

Other members of the community who may be distinguished socially from the general population are those who reside in the village but were not born there and do not belong permanently. These are outsiders with professional occupations — the schoolteacher, the *imam* or *hoca*, and the staff of the health clinic. The health and education professionals are employed by the national government, who provided and administer the school and clinic and their associated residences, as well as paying staff wages. A clear social distance is observable between the village born residents and the professional civil servants, the latter being treated with some deference and social distance.

Although most village land is worked by the villagers, there is also an area of land down by the river which is rented out for three to four months each summer to a group of beekeepers. These beekeepers are residents of Ordu on the Black Sea coast, who travel up to the highlands in the early summer of each year to feed their bees on the wild flowers of this alpine region. They arrive in early June and leave in October when the weather starts to get cold. The group consists of a man, his two sons and his

³ Dodd, 1969, p. 264

⁴ Tachau, 1984, p. 114

brother, who bring in their hives on the back of trucks, and set up a tent for their own dwelling.

Other nomadic or semi-nomadic people such as gypsies and itinerant merchants are less welcome in the village and make their camps further away from the houses. The beekeepers, however, have now been coming to Yiğittaşı for five years, and are rented a field which is considered to be unproductive and is otherwise unused. The villagers buy honey direct from the beekeepers and socialise with them, however the beekeepers bring in chickens for food as well as flour and dried goods, and eat fish from the river, so that they are economically self-sufficient.

Archaeology

The Site

Sos Höyük is a multi-period mound site discontinuously occupied for some 5000 years. Archaeological work has exposed settlement of the third and second millennia BCE on the northern flank of the mound, a limited exposure of Early Iron Age occupation on the western side and a broad area of Late Iron, Post-Achaemenid and Medieval period remains on the summit. The modern village, dug into the sides of the site, continues the sequence of occupation.

Architecture

The archaeological features most useful for an ethnoarchaeological study are those with which material culture may be correlated to ancient social and cultural norms. Whilst all ancient remains must reflect (or mirror or distort) ancient beliefs and behaviours, architecture is particularly useful in that it is always found *in situ* so to speak. The way people have chosen to order their built environment is based on prevailing ideas about needs for space, shelter, access and privacy. It can reflect environmental conditions, economic imperatives, demographic ratios and social statuses. It can also help us to understand the ancient site as it existed as a functioning community, rather than as a series of disconnected artefactual categories and typologies.

Architecture at Sos consists predominantly of stone, mud brick or stone and mudbrick constructions. Careful excavation, however, has also revealed traces of wattle and daub huts, dating to the earliest phase of settlement yet uncovered, which is the Early Bronze Age (Fig. 6). Housing styles show

links to the Caucasus, particularly the round cornered stone rooms with internal hearths, as does the ceramic repertoire and other small finds. Although no complete, identifiable room complex has yet been discovered, the Caucasian parallels suggest that these rooms may constitute single roomed dwellings.

Especially interesting are the features associated with these houses. Hearths, clay storage bins and possibly annexes (as in Trans-Caucasia) would complete a dwelling functionally identical to the parts of modern homes that are used by humans. Although the ancient houses show no evidence of attached barns or storerooms, they include a cooking area and a multipurpose room for eating, sleeping, entertaining guests and carrying out sundry tasks of child care, food preparation, craft work etc. Portable hearths increase the flexibility of living spaces still further, allowing for outdoor cooking in fine weather and indoor cooking and heating during the colder months.

As well as domestic buildings, there is evidence for a large-scale (public?) construction dating to very early in the settlement history. This consists of the very large circular wall segment found on the northern slope of the site (Fig. 7)⁵. With an estimated diameter of 15m, a wall width of 2.5m and no clearly associated floor surfaces either inside or out, this structure is totally unlike any domestic structure yet uncovered here or elsewhere in the region during this period. Its actual function is as yet unknown, but its construction and scale suggest that the work of designing and building this thing must have been carried out cooperatively by a group consisting of more than one household's members.

Other features of the site contribute to the overall functioning of the settlement, although they are perhaps less obvious than the public and private buildings. An unusual element at Sos Höyük is the circular or figure eight shaped shallow plaster basins which appear in the Early Bronze Age settlement, in some association with the round cornered rooms. The absence of walls suggests that these basins were outside. They may have been used for food preparation⁶, although no associated finds could corroborate this. Unfortunately the relationship between specific basins and individual houses is less than clear. Comparison with the courtyards of modern village houses suggests that such basins may have provided a delimited external domestic area within which household tasks could be carried out.

⁵ Sagona, A., Erkmen, M., Sagona, C. and Howells, S. 1997, p. 187

⁶ Sagona, A., Erkmen, M., Sagona, C. and Thomas, I. 1996, p. 33

Ethnoarchaeology

Ethnography and Archaeology

The ethnographic survey of Yiğittaşı village was designed and implemented with an archaeological context in mind. The specific approach taken to the survey was informed as much by the archaeological material already excavated on the site, as well as a more general archaeological epistemology, as by a purely ethnographic or anthropological methodology. Ethnographic studies are never all encompassing, and at best are able to indicate the possible transformations that occur between social behaviour and material culture. For the ethnoarchaeologist, the most important information lies in the slippage from ideology to artefact. It is necessary to be aware of how behaviour and belief are not only reflected in material culture, but also distorted and even inverted.

Tentative Reconstruction of Sos Höyük in the Bronze Age

There is no doubt that the reconstruction of a fully functioning ancient community is an impossibility. The sheer complexity of a human community, even a small, isolated one precludes a total understanding of the group by an outsider (and possibly even by those who are part of the group itself). This level of complexity is further obscured in the case of ethnoarchaeology by the biased nature of the evidence, which can only ever be partial and incomplete. Gaps in the evidence occur in the ethnographic material, the archaeological record and in the material culture which mediates the two.

Archaeological evidence of regional influence in ideas and technology, as well as the presence of imported goods indicates that Sos Höyük was part of a regional system from the earliest phase of settlement there. The environmental data suggest that the area around Sos Höyük was certainly rich enough to support a basic level of existence for a small settlement through a mixed strategy of cereal crops, animal husbandry and wild resource procurement (in the form of gathering if not also hunting). The modern ethnographic evidence too, supports the idea of a local settlement basically self-sufficient in food and shelter requirements. Although the modern settlement at Yiğittaşı is no longer completely self sufficient, the much higher population density of today suggests that the carrying capacity of the land was not exceeded in antiquity.

Modern household arrangements for economic cooperation provide a useful model for reconstructing ancient social arrangements. It is likely that

the small dwellings of ancient Sos Höyük housed not individuals, but household groups, their composition based in kinship but affected also by economic needs. Although a household could conceivably consist of a single person, the ethnographic evidence from Yiğittaş and elsewhere⁷ suggests that the basic unit be founded on a married couple and their dependents, whether children, grandchildren, in-laws, elderly parents, other relatives, such as a widow or orphaned child, and dependent clients or employees, such as shepherds. Such a group probably pools its labour and resources and shares its profits.

At a higher level of inclusiveness would be the settlement, consisting of a number of interacting households, probably more or less closely related to each other through endogamy, and interdependent both at a settlement level of division of labour and in the face of threats or competition by outsiders. It is probable that some sense of corporate identity existed at this level in the Early Bronze Age at Sos Höyük. The construction of the large circular stone wall at the very beginning of this settlement phase seems likely to have been carried out by a group larger than a single household. Although the function of this structure is still unclear, its massive scale suggests that both planning and construction must have affected and involved all members of the community.

Such corporate activity requires cooperation, if not necessarily a separation of roles and responsibilities. Although larger in scale than domestic dwellings, there is no obviously higher level of technology or skill required for the construction of the circular wall. So there is little evidence for commercial production in Bronze Age Sos Höyük. It is entirely possible that further excavation may reveal community level specialisation, such as workshops or storehouses. It is also equally likely that specialised production for exchange left no identifiable archaeological signature. This does not mean, however, that it did not occur.

One interesting possibility is that the people of Sos Höyük controlled access to land. In exchange for imported craft goods or even food or other items, the local inhabitants may have allowed access to grazing lands, or even other natural resources. Such a strategy is in place today, when nomadic or semi-nomadic people arrive on the Erzurum plain in summer to exploit local grazing for sheep and goats, and the local wildflowers for their bees. Access to these resources is controlled by the permanent inhabitants, who can profit from the nomad's need for land.

⁷ Stirling, 1965; Pierce, 1964; Delaney, 1991

It is not possible to excavate the entirety of the settlement for any period of occupation at Sos Höyük. It is possible, however, to speculate about the household level of organisation in the Early Bronze Age settlement. A combination of houses, hearths and plaster basins could be used to indicate the division of households. Basins seem to occur in association with houses of various Early Bronze Age phases, particularly in the later third millennium. It is possible that they functioned as a mediator of public versus private space, being open-air and thus visible (since they do not seem to be enclosed by courtyard walls), yet owned and utilised by individuals or known and named household groups. Thus it may be possible to identify household spaces in the association of built structures ('houses', 'rooms'), with certain assemblages of material culture, including hearths and plaster basins. A cluster of such items can form the basis for the identification of household groups in the archaeological record. This is particularly useful archaeologically, since it relies on the physical association of fixed items, rather than the fortuitous conjunction of portable artefacts which remain on abandonment of the area.

It is unlikely we will ever fully understand the complexities of a vanished culture and society. The partial nature of the archaeological record, the absence of informants and texts and the unacknowledged cultural biases of the investigators preclude full understanding. Observing a variety of modern solutions to age-old problems, however, broadens our ideas of the possible strategies adopted by those whose ancient lives we seek to understand.

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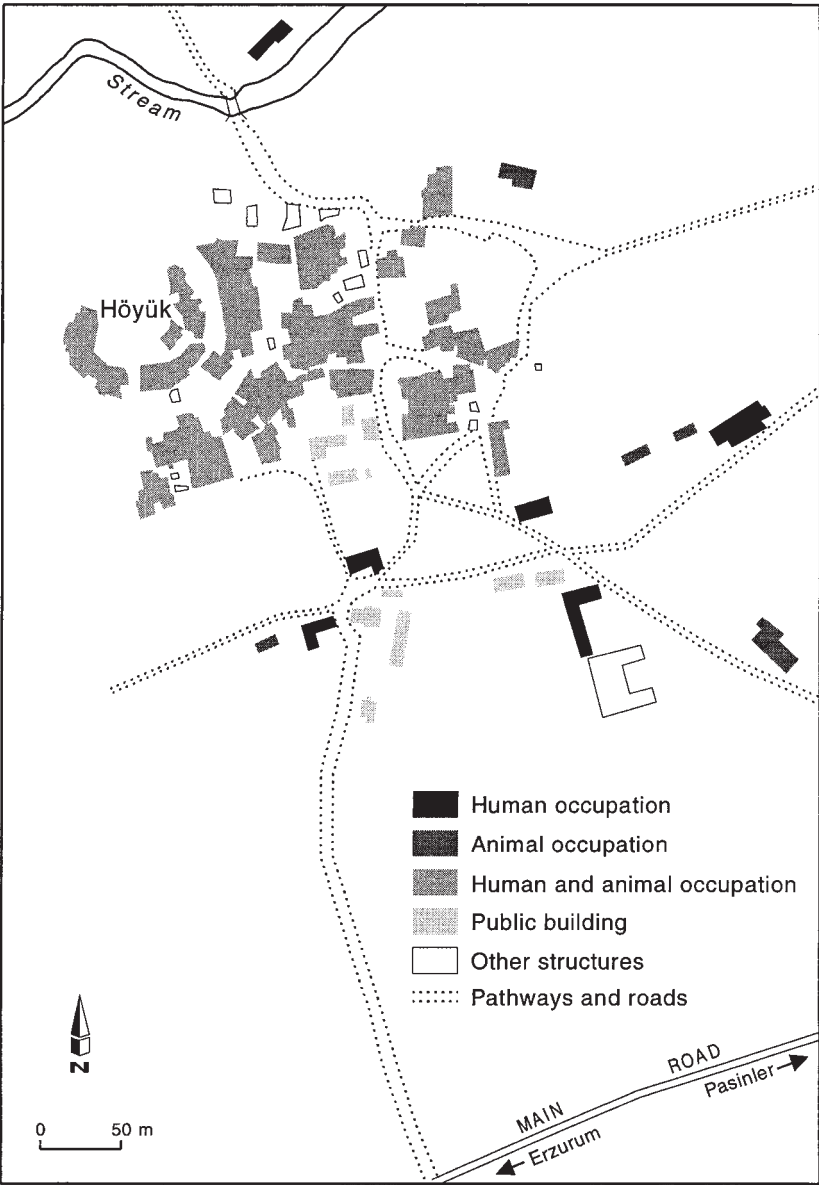


Fig. 1. Utilisation of roofed areas in Yığıttaş village.

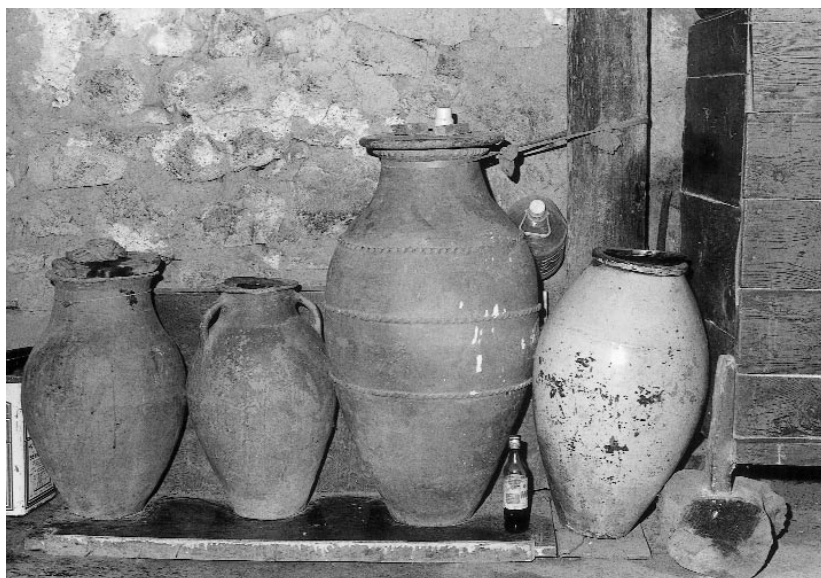


Figure 2: Modern storage vessels.



Fig. 3. Interior of a hallway kitchen, showing storage of cooking and food preparation utensils. The *tandır* is set into the platform on which the tea tray rests.



Fig. 4. Interior of a guest room showing commercial furniture.



Fig. 5. Typical village dwelling, showing cement rendered field stone walls, commercial door and window fittings, and timber and dung roof. Dung collected for use as fuel is stacked in the foreground, with more fuel stored on the roof.

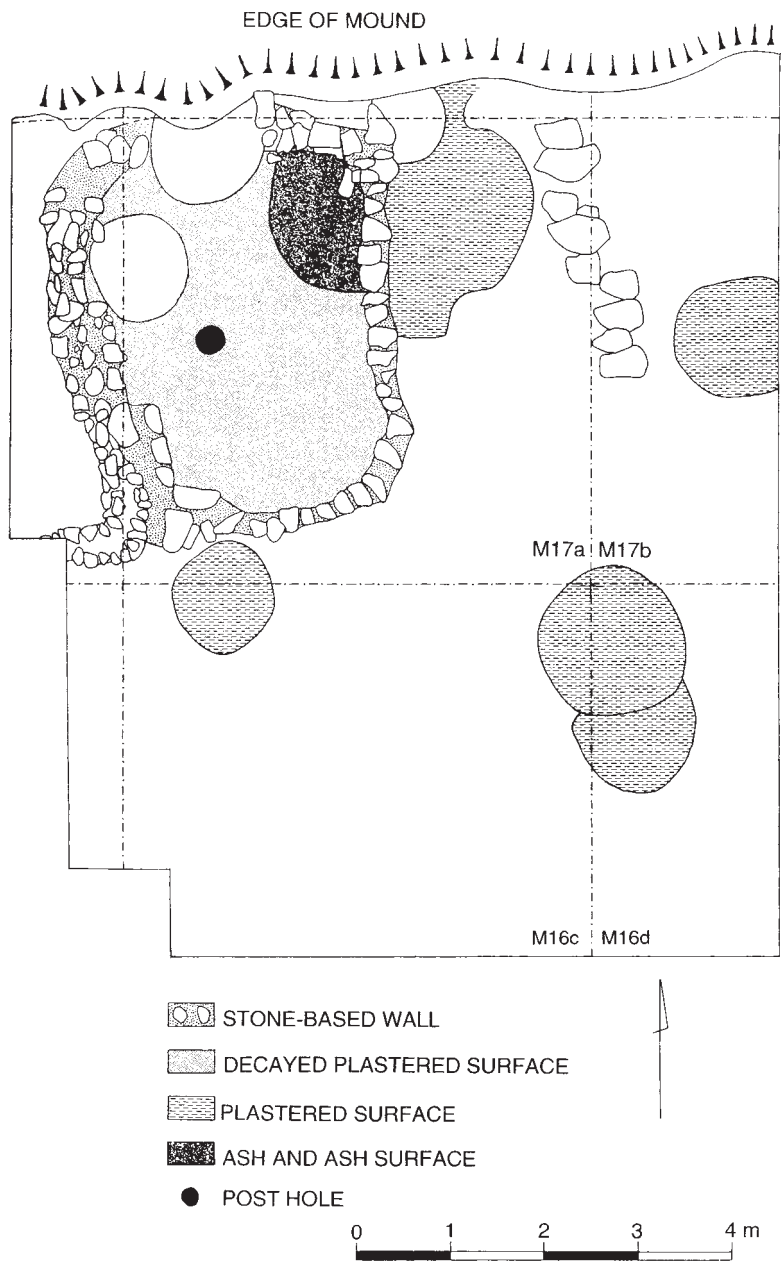


Fig. 6. Sub-rectangular dwelling datable to Middle Bronze Age I (Sos IVA) 2200–2000 BC.
Its stone footings would presumably have supported a wattle-and-daub superstructure.
Circular plaster basins are set into the adjoining courtyard.



Fig. 7. Part of a massive curved stone wall belonging to the Late Chalcolithic period (Sos VA) 3500/3300-3000 BC. Its purpose still remains unclear.

Physical Anthropological Analysis of Skeletons from Sos Höyük

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Abstract

There have been few reports documenting analysis of human skeletal remains in Anatolia, especially in northeastern Turkey where excavations have been sparse. When combined with detailed reports of burial chambers and grave goods such information may make a significant contribution to the knowledge of mortuary practices in this region.

This study focuses on one site in northeastern Turkey, Sos Höyük (excavated 1994 — present), with burials from the Early Bronze Age III Period (2500-2200 BC)

*to the Post Achaemenid Period (200-300 BC). Seven adult skeletons, five males, were uncovered. Analysis involved determination of age, gender and height and a detailed description of all degenerative, anomalous or pathological traits, including comment on possible causation. Ramifications of burial position, in relation to taphonomy, are also addressed.**

"It is paradoxical that the archaeology that has for so many years concentrated on the tombs of Cyprus has, by the same token, ignored the study of death. It is a situation which is mirrored in the ancient Near East"

Bright, 1995.

Introduction

Analysis of human burial sites has tended to concentrate on the burial chamber and associated grave goods providing a considerably better understanding of the richness and variety of previous cultures. The human remains from these sites, however, represent a direct link to past peoples; they provide demographic data concerning diet, pathology and trauma, and occupational demands. In conjunction with examination of architectural remains, they may also be able to describe cultural or ethnic links and (particularly in relation to Trans-Caucasus, Syria and Iran) shed light on the movement of the actual peoples.

The inadequacy regarding skeletal data has begun to be redressed in some geographical and temporal regions, though there are still only few reports concerning the Near East. Indeed, the organisers of the conference "The Archaeology of Death in the Ancient Near East" identified a deficit in reporting mortuary practices despite excavations of numerous burials.¹ The apparent lack of interest in the skeletal remains can, to an extent, be explained by the specialised skills necessary to analyse this material. It is also influenced by the preference of earlier excavators (many with little knowledge of human skeletal anatomy) to select only complete skeletons, thus biasing the data for future researchers.

Through a thorough physical anthropological analysis it is possible to determine an individual's age, gender and height. Previously, archaeologists have inferred these characteristics from the comparative size of bones or associated burial goods.² Although such indicators may occasionally corre-

* The project was funded in part by the Australian Research Council and the Australian Institute of Nuclear Science and Engineering. Photograph taken by Bronwyn Douglas.

¹ Campbell and Green 1995

² Bright 1995

spond with the results of a physical anthropological analysis, they require further validation. They do not represent independent data and therefore cannot be used to formulate an understanding of the demographic make up of a society. Furthermore the use of burial goods to determine gender, such as the association of females with spindle whorls and, in turn, the textile industry, highlights preconceived gender roles and social behaviour.³ Physical anthropological studies overcome these problems by having all data conform to specific and consistent guidelines.

Paleopathology and physical anthropology are particularly underdeveloped disciplines in the archaeology of Anatolia. The few anthropological studies in Turkey have provided a guide for the information available from human remains and the impact of this data on the archaeological record. Alpagut⁴ and Schultz⁵ conducted studies in this area, however neither investigated the anthropology of a region, nor combined data with the latest technology, such as DNA testing. Compared to the more extensively investigated regions further west, which have yielded large necropolis, the north-eastern zone remains sparsely excavated and little understood.⁶ Consequently, comparison of human remains and burial practices with other sites has not been possible as this region represents a distinct cultural and climatic area.

This paper presents a report of a physical anthropological study, conducted as part of the large multi-disciplinary project in northeast Turkey, directed by staff at the University of Melbourne. The project has carried out surface surveys in the highlands of eastern Turkey since 1988 as well as investigating two multi-period sites: Büyüktepe Höyük (1990-1993), in Bayburt province, and Sos Höyük (1994-present), in the neighbouring province of Erzurum. The focus of this article is the seven adult skeletons excavated from Sos Höyük burials between 1994 and 1997. These range from the Early Bronze Age III Period (2500-2200 BC) to the Post Achaemenid Period (300-200 BC).

Methodology

Analysis of bones took place between two and five years after they were excavated. At excavation skeletal remains had been collected and bagged following the procedure for all finds. No anthropometric analysis was car-

³ Keith 1998; Gjerstad, Lindros, Sjöqvist, and Westholm 1934, p. 37

⁴ Alpagut 1980-1985

⁵ Shultz 1989

⁶ Sagona 2000 for a recent history of archaeological research in this region

ried out at this stage, as a specialist physical anthropologist was not present. Thus, the first task of this study was to determine the number of individuals that had been uncovered, which is not always obvious from initial examination, after which the characteristics of each skeleton could be ascertained. Physical anthropology relies on previously established formulae and measurement of bones to determine various traits. In the case of gender determination these traits are highly dimorphic. As such they are normally associated with the pelvis and, to a lesser extent, the skull. Measurements of other bones may be used if neither the skull nor pelvis is extant, but the results are less reliable. Within the scope of this project, age was calculated using a multi-factorial approach.⁷ Because of the high degree of variability associated with age changes, analysis from multiple sources (within reason) enables the greatest accuracy. Height was calculated using the most widely accepted formulae based on a modern Caucasian population,⁸ adjusted appropriately for age and gender.

An inventory of all dentition — its stage of development, wear and presence, if any, of periodontal disease — was taken. Evidence of pathologies, trauma, anomalies and ante-mortem degradation of all skeletal material is an important factor in determining the physical stresses of a population and aspects of cultural behaviour. A total of twenty-four pathological conditions that exhibit distinct skeletal changes and any abnormal characteristics, that may later be attributable to other pathologies, were recorded.⁹ Skeletal and dental changes due to age have been reported under pathologies. DNA testing is currently being undertaken to ratify pathological findings and, in the future, elucidate possible familial relationships.

Formulae, such as those used to determine height and age, may be limited because of their derivation from modern populations exhibiting differing diet, environmental and work rigours. Nonetheless, allowances can be made (*e.g.* for diet) and, as increasing numbers are analysed in each population, the variation between the sample and original group may be gauged. Consequently, the methods described are the most accurate and reliable at the time.

⁷ Based on an altered form of Lovejoy, Meindl, Mensforth and Barton, 1985. This study used age-related changes to the (1) pubic symphyseal face (2) auricular surface (3) sternal rib end (4) ectocranial sutures (5) epiphyses (6) dentition.

⁸ Trotter and Glesser 1952; Trotter and Glesser 1958

⁹ Ortner and Putschar 1985

Burials

Early Bronze Age III

The Early Bronze Age at Sos Höyük lasted from 3000-2200 BC, but to date, burials have only been found in the Early Bronze Age III (EB III) or Sos Vd period (2500-2200 BC).¹⁰

Burial 3, Trench M16

The first burial, Burial 3, in Trench M16 (excavated in 1997), was a simple pit containing a flexed skeleton, a black burnished bowl and 2 shell rings.¹¹ Culturally, this interment belongs to the Kura-Araxes complex and, as such, assumes some significance. Despite the literally hundreds of sites that have been ascribed to the Kura-Araxes period across the highlands of eastern Anatolia and Trans-Caucasus, the concomitant number of burials are remarkably few, especially in Anatolia, where the recent discovery of the spectacular burial at Arslantepe is most prominent.¹² In Trans-Caucasus, simple pit graves, sometimes stone-lined, have been recorded at Elar, in Armenia, and at Samshilde, in Georgia.¹³

The post-cranial skeleton had been well preserved and was very nearly complete, although the cranium and mandible had been subject to far greater destruction. Numerous bones from a second individual were also discovered. Criteria for allocation of bones to a different person must always be stringent; therefore the designation of two individuals is the most conservative estimate. It is certain that one calcaneus, talus, third metacarpal and a metatarsal represented a second individual.

Traits

Sex was primarily determined using the pelvis, owing to the inadequacy of data from the skull. From this it was apparent that the specimen was most likely male, although no pelvic features were strongly indicative of either sex. This supposition was ratified by measurement of the glenoid cavity (scapula) which (on the basis of modern data) positively identified a male specimen.

¹⁰ Sagona 2000

¹¹ For a complete description of Burial 3, Trench M16, see Sagona, Erkmen, Sagona, McNiven and Howells 1998, p. 33

¹² Frangipane 1998

¹³ Khanzadian 1979; Mirtskhulava 1975

The most specific and accurate age determinant was the third molar. The left mandibular molar had formed, but was yet to erupt and therefore limits the age to between 19 and 23 years.

Height was calculated from the tibia and determined to be between 166.50 cm and 174.50 cm.

Pathologies

Only one section of the mandible (about one third) was located. Nine mandibular teeth were extant, five of which were in their sockets, the remaining four were loose, but able to be replaced. All teeth showed significant calculus build up on both their lingual and buccal surfaces. This was not only localised to the neck (where it was greatest), but extended to the biting edge on the buccal surface of the two incisors and one canine. Wear was minimal or non-existent on the left incisors, canine and premolars and only three small patches of wear were apparent on the cusps of the first and second molar. Conversely, wear of the right incisors and canine was extensive and had resulted in a bucco-lingual slope of approximately 45 degrees. This pattern of wear is inconsistent with normal masticatory wear and may suggest adoption of a particular task-related bite or mandibular posture.

Both maxillary molars showed signs of considerable periodontal disease, quite possibly related to the ante-mortem loss of neighbouring teeth. Resorption of alveolar bone had extended to the region superior to the first molar. Extensive caries were apparent on the occlusal surfaces of both these molars and there was heavy calculus build up around the necks. Both teeth were almost completely devoid of enamel with exposure of the dentine on the lingual and buccal surfaces. Enamel was only present on the cusps of the teeth, forming a “frill” when viewed from the front. This type of enamel loss is unlikely to be attributed to normal wear (which typically targets the occlusal surface) and as the enamel was still present on the cusps of the teeth, post mortem degradation is unlikely. Thus, periodontal disease would appear to be the only explanation. Unfortunately, as no other maxillary teeth were extant the extent of this wear pattern cannot be gauged.

Other pathological conditions were also noted. The cervical spine showed extreme degenerative change, with ‘lipping’ of the vertebral body and osteophytic growths, greatest in the upper vertebrae. These extended to the odontoid process of the second cervical vertebrae (C2) where the ligamentous attachments (ligaments that control range of movement of the skull on C1 and C1 on C2) appeared to be abnormally developed. This may have been due to abnormal articulation between the odontoid process of

C2 and C1. There was slight to medium anterior-inferior lipping of the bodies of all other cervical vertebrae as well as an increasing porosity of these bodies. Neither the vertebral canal nor transverse foramen were compromised although a costo-transverse bar was present in the right vertebral artery foramen at the level of C5.

Examination of the thoracic spine also revealed pathological change. The body of the fourth thoracic vertebra (T4) exhibited mild compression resulting in a loss of integrity of its superior surface and callus formation supero-laterally. Whether this was due to compression fracture or degeneration is uncertain. The spinous processes of the thoracic vertebrae, between the levels of T4 and T9, were also abnormally aligned. This may have been a compensatory response to the trauma at T4 leading to osteophyte generation on the left costo-vertebral articulations of T7 and T8.

The only other abnormality seen in this skeleton was osteophytic growth at both the distal and proximal ends of the fibula. Slight changes were also apparent on the tibia where these two bones articulate. A possible explanation may be dislocation of the fibula with respect to the tibia, resulting in bone remodelling and the formation of a pseudoarthrosis.

Squatting facets were present on both tibias. Squatting facets are indicative of habitual movements, specifically extreme dorsi-flexion at the ankle. They are formed when the distal joint surface of the tibia extends anteriorly onto the upper surface of the talus. The skeletal manifestation of this is an increased surface area for articulation, effectively forming squatting facets. This trait was detected in three of the seven skeletons at Sos Höyük suggesting a common link manifesting itself as the adoption of a particular posture. Squatting facets are a non-metric variation and are not genetically linked, but rather reflect day-to-day work patterns and are common in indigenous peoples who work or rest in a squatting posture.

Burial 1, Trench M15d

The second EB III grave, Burial 1, Trench M15d (excavated in the same year), was a 2 m deep shaft with a partially disarticulated skeleton, black jar and a white shell ring.¹⁴ At this stage, we see no reason to alter our original designation of 'Proto-Trialeti' for this burial. The type of interment, the style of the grave goods and the radiocarbon date of the skeleton itself all

¹⁴ For a complete description of Burial 1, Trench M15d see Sagona, Erkmén, Sagona, McNiven and Howells 1998, p. 34

point to a phase of cultural development prior to the well known Trialeti burials of Trans-Caucasus.¹⁵

The post-cranial skeleton was reasonably well preserved, while most of the cranium had not survived in antiquity. The right half of the mandible did remain and contained four teeth.

Traits

The skeleton was deemed to be male, primarily because of characteristic features of the right hip bone. The small section of skull, which includes the supra-orbital margins and glabella, support this theory, although these features were so extreme as to suggest racial contributions as well as gender.

Age related changes on the auricular surface of the hip bone suggested the individual was between 50 and 60 years at the time of death. This was corroborated by observation of tooth wear.¹⁶

Height was determined from the tibia, as the femurs were incomplete, and the result adjusted for the advancing age of the specimen. The resultant stature range was between 175.65 cm and 183.65 cm.

Pathologies

Advanced osteoarthritic changes were evident throughout the skeleton, particularly in the sacrum, resulting in a bony lip projecting 5-6 mm from the superior surface of the body of the first sacral vertebra.

Slight remodelling was apparent on the posterior surface of both patellae with the right bone also exhibiting slight compression. The only other site of degeneration was the right temporomandibular joint, a common location for age related changes.

This skeleton exhibits certain other non-pathological features. A striking attribute is the individual's great stature. Male population stature for Sos Höyük ranges from 164.87 cm (± 1.46 cm) to 173.49 cm (± 0.74 cm). These figures are calculated from a small group ($n=4$), however it is worthy of note that the least possible predicted height for 1 M15d is still two standard deviations above the maximum height for this population.

Compounding this individual's larger stature are distinctive facial features. The supraorbital ridges and glabella are highly developed producing a prominent brow. Furthermore the general weight and robusticity of the bones is greater than any other specimen.

¹⁵ Khusnareva 1997, pp. 89-114.

¹⁶ Brothwell 1981

Squatting facets were again present.

Another trait that may be deduced from skeletal remains is handedness. In the case of 1 M15d, the differences between the bones of the left and right upper limb (right being larger) were extreme. Whether this was the result of occupational demand or stature related is uncertain.

Middle Bronze Age I

The Middle Bronze Age I (MB I) or Sos IVa is denoted as lasting between 2200-2000BC.¹⁷ Three burials have been found from this period; two of these contained human remains.

Burial 1, Trench M16

The first burial, Burial 1, Trench M16, was excavated in 1996 and contained bones in a small stone-lined pit which had been positioned directly above burial 3 M16. Associated grave goods included a bronze lunate hair-ring and black incised jar of Trialeti type with flat capping stone.¹⁸

All skeletal remains were very poorly preserved with only one long bone (right radius) surviving intact. The skull, although in fragments (14 pieces) was still able to be reconstructed.

Burial 1 M16 (like 3 M16) revealed a section of the temporal bone (external auditory meatus, styloid process and mastoid process) and the second cervical vertebra from a second individual.

Initially it was thought that the extra bones simply represented two skeletons (1 M16 and 3 M16) and that disturbance of the burials had caused commingling. However, after all bones had been analysed it was discovered that at least one of the bones (and possibly more) belonged to a third skeleton (see Discussion).

Traits

Gender was determined using two characteristics of the pelvis and the transverse and vertical diameters of the humeral head.¹⁹ Although so few traits cannot ensure complete accuracy, all four were highly suggestive of a male specimen.

¹⁷ Sagona 2000

¹⁸ For a complete description of Burial 1, Trench M16 see Sagona Erkmén, Sagona and Howells 1997, p. 185

¹⁹ France 1995

Age determination was also difficult, as three of the six traits were no longer present. Nonetheless, two of the remaining indicators (auricular surface and tooth wear) suggested an age range of 25-35 years. As the auricular surface is less susceptible to population influences this age (30-34 years) was considered most accurate.

Finally, height was determined using the right radius and estimated as 163.80 cm -173.20 cm. (The radius provides a less accurate determination of height than the bones of the lower limb therefore the range is greater.)

Pathologies

Due to the comparatively young age of this skeleton it was anticipated that no degenerative changes would be found. However, osteophytic growths were evident on the right bifid portion of the C2 spinous process, with all other cervical vertebrae also exhibiting some degree of lipping.

The relative completeness of the skull ensured the majority of the dentition was still present. The 16 mandibular teeth were able to be positioned in their sockets. Calculus was present on all teeth, localised around the neck region and particularly prevalent on the incisors. These teeth had been pushed posteriorly and laterally due to 'crowding' of the jaw. Wear patterns on the occlusal surfaces of the molars and premolars were within expected limits except on the right mandibular molar where dentine was exposed on all four quadrants and large pits had formed. Atypical wear was noted on the right medial incisor and the right third molar. Both teeth exhibited wear patterns on the lateral surface at the root of the neck consistent with repeated abrasion. The wear did not exhibit the same aetiology as carious lesions that might also be expected at this level. Finally, the left molar was impacted.

Wear of the maxillary teeth was less extensive than that on the mandibular ones, with less calculus build up, mostly localised to the posterior surfaces of the incisors and canines. Wear was greatest on the first and second molars, with both first molars exhibiting deep pits. The right first and second molars also had small caries present.

An abscess was present at the site of the right upper lateral incisor (lost ante-mortem). This had caused resorption of bone up to 12 mm above the gum line, extending 9 mm posteriorly into the maxillary sinus. Such a spread of infection into the sinus system is likely to cause considerable pain, perhaps even leading to debilitating or fatal conditions.

This skeleton also showed evidence of squatting facets.

Burial 2, Trench M16

The second MB I skeleton, 2 M16, (also excavated in 1996) was from a 1.75 m shaft grave. The placement of the skeleton and presence of toggles at hand and foot suggested the body had been bound at the time of inhumation. An assemblage of Trialeti type grave goods included a black incised jar, a hollowed Y-shaped segment of deer antler, a gypsum bead necklace and the bones of a large, immature animal.²⁰

Again the skeleton was very poorly preserved, especially the bones of the skull, trunk and upper limbs.

Traits

The left innominate provided no information on gender, but four traits from the right hip bone and one from the skull were all clearly indicative of a mature adult female aged 18-20+ years.

Height was calculated from the tibia, and estimated as 160.70 cm to 168.70 cm.

Pathologies

The left pubic bone was very narrow and fragile and appeared to have suffered localised resorption, although the extent could not be fully determined due to its fragmentary nature. Similar bony changes can be linked, in modern samples, to oestrogen levels which increase as a result of parturition.²¹ Nonetheless a direct correlation between resorption pits and child-birth is yet to be established.

Post-Achaemenid

The most recent burial (Area B, burial 13) is dated at 300-200 BC or Sos IIc, revising the initial cultural designation of this burial from the Hellenistic to the Post-Achaemenid Period.²²

The grave was described as a stone-lined cist with a rhyolite burial chamber within which the remains of three individuals were discovered. The initial reporting of two individuals from this grave was revised during the

²⁰ For a complete description of Burial 2, Trench M16, see Sagona Erkmén, Sagona and Howells 1997, p. 185

²¹ Tague 1988

²² Sagona, Erkmén, Sagona and Thomas 1996

anatomical analysis. (Confirmation of the archaeological age of the third individual is pending.) A silver drachma was found under the hip of the two most superficial skeletons. A pair of silver earrings was also found alongside the deeper of these two skeletons.²³

Skeleton 1, Area B, Burial 13

The remains of the first skeleton were very well preserved, enabling detailed analysis.

Traits

Analysis of the pelvis, confirmed by traits present on the skull, suggested a male. Age was determined primarily from the pelvis. Both the pubic symphysis and auricular surface indicated an older person (27-66 years and 60+ years, respectively). Dental wear supported this. Height was calculated from the femur as between 167.10 cm and 175.00 cm.

Pathologies

Nearly all changes could be attributed to the advancing age of the individual at the time of death. Most were associated with the spine. Vertebrae from all three sections showed a characteristic "fish" shape, indicative of extreme scoliosis. This had resulted in extensive lipping, noticeable porosity, and degeneration of cortical bone. The most affected vertebrae were between T₃ and T₆ where the collapse of vertebral bodies had resulted in fusion of T₃ to T₄ and T₅ to T₆.

The cervical spine also showed extreme osteoarthritic change. Here, osteophytic profusion had occurred to such an extent as to partially occlude the inter-vertebral foramen (outlet for nerve roots as they contribute to the formation of a spinal nerve) between C₃ and C₄. The right superior facet of C₄ and corresponding inferior facet of C₃ had been remodelled by bone growths resulting in an uneven articular surface. Lesser degenerative changes were also seen on the olecranon of the right ulna, but associated changes to the humerus were not present.

General observation of the skeleton noted well-developed markings for muscle attachments. This would suggest that, even though this man was

²³ For a complete description of Area B, Burial 13 see Sagona, Erkmen, Sagona and Thomas 1996, p. 31

older and beginning to suffer the effects of age related degeneration, he was still active and participating in significant weight-bearing tasks.

A metopic suture, the suture between the two halves of the frontal bone, was also apparent. This anomaly is not pathological and although fusion is normally complete by 1 or 2 years of age, it is still visible in <1% of adult skulls.²⁴

Skeleton 2, Area B, Burial 13

The second skeleton excavated from Area B, Burial 13 was located directly under the male skeleton in what was originally assumed to be a double burial. This skeleton was not as well preserved as the skeleton found above it, although the skull was nearly complete.

Traits

The pelvis was clearly that of a female. Dorsal pitting was present on the posterior surface of the pubis. This is often presented as evidence of pregnancy though, in itself, it is not conclusive.

The pelvis was also utilised for age determination and indicated an age range between 45 and 60 years, comparable to her companion in the grave.

Stature was calculated from the femur and ranged from 154.81 cm to 162.69 cm when adjusted for advancing age.

Pathologies

Like the male skeleton osteoarthritic changes were apparent in the spinal column, though they tended to be localised around the lumbar and thoracic regions.

The left nasal bone had been compressed medially although no evidence of fracture was noted. This type of deviation is common due to the weight of soil that accumulates within the skull post-mortem, but the positioning of the bones in relation to the sutures suggested that the anomaly had occurred before death.

Significant periodontal disease was also evident. The left and right first and third mandibular molars were all carious; the first left molar being the greatest affected as the caries covered nearly half the occlusal surface. The

²⁴ Metopic sutures persist in <1% of racial groups other than Mongoloids according to Brothwell, 1981.

infection from this lesion had spread, resulting in abscess formation involving the root of the tooth; the surrounding bone had been resorbed leading to exposure of these roots. The right maxillary first and second molars had been lost ante-mortem, presumably as a result of periodontal disease or trauma, as no evidence of ritual removal of teeth has been discovered. The third molar appears to have been congenitally absent. Seven other teeth were found. None of these could be confidently re-positioned yet they all exhibited similar signs of wear and dental decay.

Skeleton 3, Area B, Burial 13

Finally, the remains of a third individual were discovered during the anatomical analysis of remains from Area B, burial 13. Compared to the proportion of the skeleton still present in skeletons 1 and 2, it is most probable that skeleton 3 represents the original burial which was subsequently 'cleared out' (removing a number of the bones) to make room for the double burial of skeletons 1 and 2. This process has been reported in numerous other European societies.²⁵

Traits

The only bones remaining were the long bones of the left upper limb, a section of the left femur and fibula as well as sections of both clavicles and left scapula. Other parts of a long bone shaft, short bones and ribs were also recovered, but were unable to be positioned. No skull bones were extant. Age could not be specifically established although as all epiphyses had closed the individual was older than 23 years at the time of death.

This skeleton was male (determined from the transverse and vertical diameters of the humeral head).

Height, calculated from the radius, which had suffered a post-mortem break, but could be reconstructed with minimal disruption to the length of the bone, ranged from 163.45 cm to 172.77 cm.

Pathologies

Due to the small number of bones excavated for this skeleton no pathologies were recorded.

²⁵ Metcalf and Huntington 1991; Morris 1987.

Discussion

This report has detailed the varying preservation of skeletal remains, from individuals, that have survived for up to 4,500 years. The extent of this is dependent upon a number of variables²⁶ which would have contributed, to a lesser or greater extent, to preservation of the Sos Höyük remains.

Soil condition (the comparative acidity or alkalinity) would not have been a significant factor as all remains were removed from a relatively small, uniform area. Climatic temperature at the time of death, age and sex of the remains and pre-existing pathologies may be significant, especially where extra bones have been discovered. This was the case with two burials: 1 M16 and 3 M16. Evidence of a second individual in Burial 1, Trench M16, suggested bones from an inhumation above (in this case 1 M16) being displaced downwards. However, all extra bones were already present for 1 M16 skeleton. Conversely the bones may have been from a burial beneath 3 M16, but as no other remains were present this, again, is unlikely. A third alternative may be that 3 M16 represents a prolonged burial, common in cold and permafrost climates.²⁷ (This is different from a secondary burial, where the corpse is interred and allowed to decompose before the disarticulated bones are collected and reburied.) In such areas a corpse may have remained above ground until inhumation could take place (perhaps until the ground thawed sufficiently to enable a grave to be dug). However the problems of storing a body in this manner for a transient community and the further complications of storing multiple bodies are yet to be addressed. It is possible that the extra bones in 3 M16 present an individual who was subject to a prolonged burial at the same time and place as 3 M16. Subsequently small bones from one corpse could conceivably have become mixed and be attributed to the other at the time of final inhumation.

In cases of prolonged burial the body experiences at least one freeze-thaw cycle, thus speeding up the decay and disarticulation processes though not disrupting the sequence of disarticulation. Studies have shown that freeze-thaw animals decay from the "outside in"²⁸ thus extremities, such as the extra bones found in 3 M16, may have disarticulated at an earlier stage. The temporomandibular joint is reportedly the first joint to disarticulate, but as this only separates the mandible from the cranium it is more likely to be re-

²⁶ Haglund and Sorg 1997

²⁷ Prolonged burials are defined by Simon 1992 as "an interment which was preceded by some passage of time after death of an individual and before final inhumation."

²⁸ Micozzi 1997

positioned at the time of inhumation.²⁹ The same would be the case for the long bones of the arm compared to the small bones of hands and feet. The second area to disarticulate is the cervical spine. This may provide an explanation for the orientation of the skull in skeleton 1 M16.³⁰ The cervical vertebrae showed no evidence of ante-mortem trauma, but had the body been stored before inhumation, allowing time for the ligaments of the cervical spine to decompose, this may account for the appearance.

The process described above may also help explain the unusual presentation of forearm bones for the skeleton 1 M15d (Figure 1).³¹ These bones appeared to be ritually displayed. However, all other bones were present and in their correct anatomical position suggesting that skin, or at least ligaments, had still been intact when the corpse had been interred. As the humeri, radii and ulnae did not exhibit evidence of cut marks at the associated joint surfaces, the freeze-thaw cycle of disarticulation provides an alternate hypothesis to removal of the forearms by cutting.

Finally, the different rate and sequence of decomposition of a corpse may be explained by its position prior to final inhumation. For example, de Laguna³² has suggested that positioning of the body, with hands and feet elevated, may result in desiccation of these parts. Subsequently the effective mummification could support bones which, if orientated differently would disarticulate.

This paper has attempted to illustrate what a detailed physical anthropological analysis of human skeletal remains can achieve. Demographic characteristics of individuals, trauma and pathological changes and occupational demands highlight the advantages of having a physical anthropologist present during the excavation. Considerable research is still required in this field as well as study in the region of northeastern Turkey to enable the most accurate representation of past populations. Final analysis might then combine with the archaeological study of burials to present a complete picture of the cultural mortuary practices of these peoples.

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²⁹ Ibid

³⁰ Sagona, Erkmen, Sagona and Howells 1997, p. 185

³¹ Sagona, Erkmen, Sagona, McNiven and Howells 1998, p. 34

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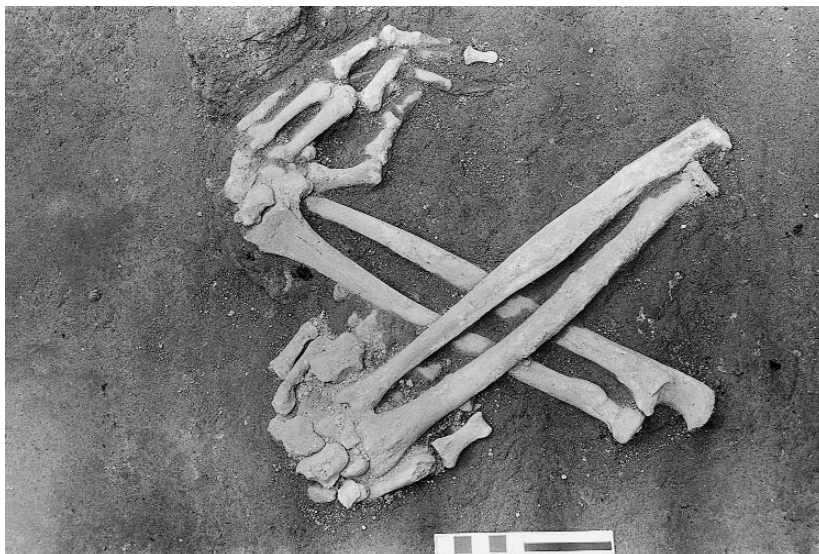


Fig. 1. Crossed forearm bones positioned in Burial 1, Trench M15d.

NOTES AND DISCUSSION

The Meaning of אַנְתָּא in the Babylonian Talmud

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The word אַנְתָּא is attested once in the tractate Menahoth 78a of the Babylonian Talmud in the phrase אַימָא אַנְתָּא דְּמִישְׁחָא “Perhaps a bread of oil?”. M. Jastrow defines the term as ‘essence, substance’.¹ L. Goldschmidt in his German translation of the Babylonian Talmud argues that the meaning of אַנְתָּא is obscure, and that the traditional meaning of אַנְתָּא דְּמִישְׁחָא as a cake of congealed oil is “absurd”. He further argues that the word אַנְתָּא could be misspelled and that it should be connected to Syriac *ʿgt*, a shortened form of *ʿgnt* ‘flat dish’, where ʿ is written instead of ʾ.² I. Epstein in the English translation of the Babylonian Talmud remarks in a footnote that, “The meaning and etymology of this word אַנְתָּא are both doubtful”.³ J. N. Epstein suggests the Syriac word *hnt* which is only attested in lexicographical dictionaries.⁴

Three magic bowls written in Aramaic square characters, and composed in different Eastern Aramaic dialects⁵, shed new light on the meaning and etymology of אַנְתָּא. In one bowl which is composed in Koiné Babylonian Aramaic and has been partially published by C. H. Gordon one finds the following passage: בסליה דלחמא בחצביה דמיא ב'ח' צב'י ה דמיש[חא] “in a

¹ Jastrow 1950, p. 88b.

² Goldschmidt 1933, p. 679 n. 47.

³ Epstein 1948, p. 467 n. 1.

⁴ Epstein 1983, p. 37.

⁵ See handbook by Müller-Kessler 2000.

basket of bread, in a jug of water, in a vessel of oil" (BM 91776, a11).⁶ A second bowl, written in Standard Literary Babylonian Aramaic, and also published by C. H. Gordon as well, has a similar phrase, ... **בסלא דלחמא** ... **במנא דמשחא** **ומנא דמא** (ZRL 48, 10).⁷ The last bowl which is the only one known thus far in Talmudic Aramaic has the following passage: **בסליה** ... **בחצביה דמא** ... **באנותיה דמישחא** (BM 135563, 11).⁸ It is obvious that the word **אנותיה** in the Talmudic Aramaic bowl is the same as in Menahoth 78a and must have a meaning similar to **מנא**. The fact is that **אנתא** is probably identical to Akkadian *unūtu* which has the meaning "vessel, instrument, garment".⁹ The root *'ny* is attested in Ugaritic, Hebrew and Arabic with slightly different meanings.¹⁰

Therefore one may conclude **אנתא** is an Akkadian loanword and the passage in Menahoth 78a **אנתא דמשחא** should be translated as "a jar of oil".

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⁶ Gordon 1941, pp. 342-344. The last two words are missing from Gordon's transliteration.

⁷ Gordon 1978, pp. 231-244.

⁸ See forthcoming article on the cultural background of the formula and the dialect of bowl BM 135563 by Müller-Kessler and Kwasman 2000 [in press].

⁹ The listed Mandaic word *'n't'* occurring in magic bowl 38:3 in Montgomery 1913, pp. 244-246 has to be corrected to *'n'šy'* (collated reading).

¹⁰ von Soden 1981, p. 1422.

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A New Suggestion for the Hebrew Hapax Legomenon סַאון from Qumran

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In the latest issue of *Abr-Nahrain* T. Muraoka continues his *notae qumranicae philologicae* on the Community Rule (1QS) dealing with column 3. Among the problems he discusses is the passage from 1QS column 3, 2 בַּסַּאון רַשַׁע מַחֲרָשׁוֹ. The סַאון is an unsolved *hapax legomenon* in the Qumran Hebrew text corpus. The Biblical Hebrew dictionaries offer only the meaning “shoe” for סַאון in Isaiah 9:4 which means there “shoe”. It is obviously not a suitable meaning for the Qumran homonym in 1QS column 3, 2. Helpful in this context is the parallel word גִּוְאֵלִים for which various different translations have been suggested “muck, mud, slime, filth, stain”.¹ Thus one has to assume that the word סַאון has a similar meaning as well. Since the Hebrew Bible corpus does not help with a solution further lexicographical explanations could be found in the dialects of Western Aramaic.

In this case Christian Palestinian Aramaic should be scrutinized, since many Hebrew loanwords have survived in this dialect. Most of those Hebrew loanwords are well known², but some of them are very rare or are not attested in Biblical Hebrew, e.g. *LWK* “to soil”³ which appears only in Middle Hebrew. The new edition of the Corpus of Christian Palestinian Aramaic texts from the early period confirms many lexicographical peculiarities.⁴ It

¹ Muraoka 1998, p. 49.

² See Jacob 1901, pp. 83-113.

³ Müller-Kessler 1991, p. 8.

⁴ Müller-Kessler and Sokoloff 1996-1999.

has been one aim of this text series to establish a reliable text bases for new lexicographical and grammatical studies. Until recently many fragments were only partially published, some were left unpublished and many previous editions carried reading mistakes. This is also true for the last volume of Cyril of Jerusalem of which a third of the surviving text was unedited.⁵ This text contains three interesting text passages which might throw light on the understanding of סִאָן. All three passages were not correctly read by J. P. N. Land.⁶ The first passage from catechesis 6:34 has 'sy'n' *wsrywt* translating Greek βορβόρων, another passage from the same chapter reads *dl' th' mtml' mnh mn 'sy'n* "so that you do not get soiled by it", and the third attestation is 'syn' *byš* "unrefined clay" catechesis 13:11 translating probably τῆς συρφετώδους ὕλης. 'sy'n' has been one of the mystifying words in the Christian Palestinian Aramaic version of Cyril of Jerusalem. We have tried to explain it as a word based on Greek ἄσις, but this has not been a very satisfying explanation. It is highly likely that our Christian Palestinian Aramaic word 'sy'n' with an prosthetic ' and diacritical dot on it corresponds to the Hebrew word in the Qumran text סִאָן. Apart from the Christian Palestinian Aramaic attestation there exists an Eastern Aramaic word *syn* "mud" which is attested in Syriac, Talmudic Aramaic and Mandaic. Interestingly, the latter has in the Modern Mandaic pronunciation a prosthetic vowel *esyâna*.⁷ The defective spelling in the Eastern Aramaic dialects disguised the obvious solution for the Christian Palestinian Aramaic variant. Therefore one may conclude that (')s(')n' is a common Aramaic word and not a Hebrew loanword. (')sy(')n' backs up on the one hand the meaning of סִאָן which was already translated correctly by several scholars in connection to the parallel גִּזְאִים, but on the other hand it does not explain its etymological background.

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⁵ Müller-Kessler and Sokoloff 1999.

⁶ Land 1875.

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REVIEWS

J. Elayi et A. Planas Palau, 1995, *Les pointes de flèches en bronze d'Ibiza dans le cadre de la colonisation phénico-punique*, Transeuphratène supplement no. 2, Paris: Gabalda. Pp. 350, Figs 43, Pls 25. ISBN 2-85021-078-1. Price: 350 F.

By Claudia Sagona.

Elayi and Planas Palau have presented both a thorough catalogue of 96 previously unpublished arrowheads from Ibiza and devised a typology comprising 31 shape categories (Chapters I-III). Concise essays on geographical distribution (Chapter IV) and chronology (Chapter V), methodically and systematically address each type. The authors have also documented a large amount of comparative material from lands bordering the Mediterranean and east, into central Asia. Origins, trade, production, and usage for each category are discussed, in turn, in Chapters VI-VII. The intrinsic limitations involved in such a study are clearly outlined by the authors and conclusions are kept within reasonable boundaries that can be supported by the evidence (*e.g.* p. 186).

Given that the value of works such as this lies in the publication of new material, it is a great pity that more effort was not taken with the illustrations. The line drawings of the artefacts mar the overall impact of the publication. While the photographs of the arrowheads themselves are clear, the images have been visually overwhelmed by the graph background; a technique for representing the scale of objects to be avoided at all costs. The production is also not without flaws: important headings are indistinct (especially categories I-V on pp. 19-23) and there is a loss of clarity in lettering and punctuation (at least in my copy).

Figs 2-6 illustrate the key to the five features ('extrémité, corps, base, anm-manchement, accessoire') documented for each arrowhead in the catalogues — the new points from Ibiza (Chapter II and Appendix, pp. 261-272), and previously published arrowheads (Chapter III). It is never an easy task with typologies to decide how far to subdivide the categories. I am not convinced that all features warrant the often subtle degree of categorization, considering that these objects were functional and, as such, were subject to use wear and modifications resulting from sharpening and repairs, as well as corrosion over time (note for example the distinction between profiles such as II2.C.2.b and c, or base III.B.3.a and b).

The same reservations apply to the 31 typological forms the authors present in Chapter III and illustrate in figs 7-10. Again the distinction in a few categories is not great enough to warrant separate types [*e.g.* fig. 7: III-VI, or 9: XXII-XXV].

The order of the presentation of material is also jarring. It would be more logical to present the Typology (Chapter III) after Chapter I, where the terminology of the study is defined, and before the catalogue (Chapter II), given that the typology establishes the categories used in the catalogue: a case of the cart going before the horse. A technical problem arose with the late addition of arrowheads nos 86-96, that presented a new category, Type XXXI, fig. 10. This type is discussed only in the Appendix (p. 261). The missing number on the last plate is another minor oversight.

At 350 F, the reader could rightly expect a better quality production (it has to be noted that more recent Gabalda publications are of a better standard). The strength of this work lies in the detailed presentation of new data and its extensive bibliography that provide excavators and researchers with a valuable resource with which to access a widely distributed artefact.